MEMOIRS OF THE LITERARY AND
PHILOSOPHICAL SOCIETY
OF MANCHESTER.

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OF THE

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LITERARY AND PHILOSOPHICAL SOCIETY OF MANCHESTER.

I. *Observations of Comet* I. 1861.

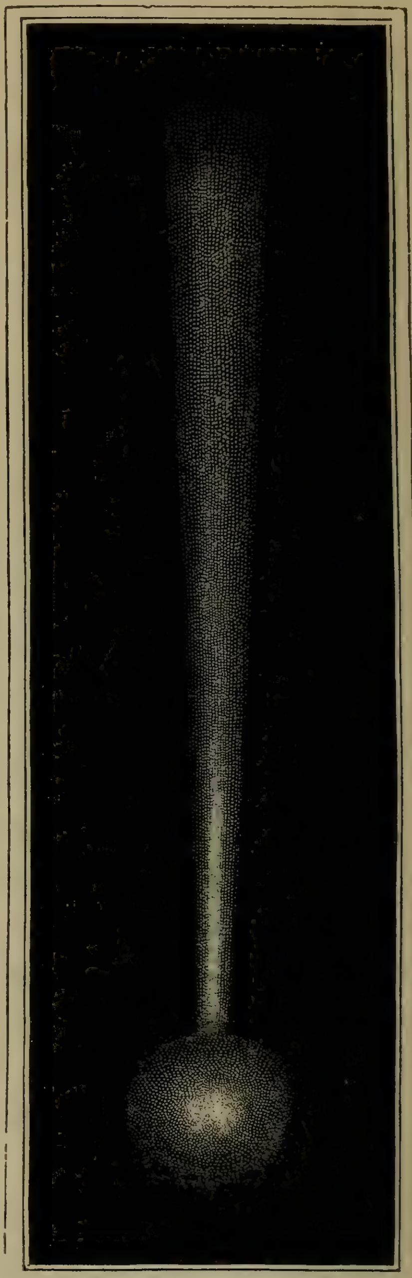
By JOSEPH BAXENDELL, Esq., F.R.A.S.

Read October 1st, 1861.

ALTHOUGH this comet was not at any time a very conspicuous object to the naked eye, yet some of the features which it presented when viewed with a good telescope at the time of its greatest brightness were sufficiently remarkable to render it an object of peculiar interest to the astronomer; and I have therefore thought that a brief account of the observations made with the excellent instruments of Mr. Worthington's observatory might be acceptable to the members of this Society.

My first observation was made on the night of May 3rd, 1861. The comet was then already visible to the naked eye as a dull, hazy-looking star of the $4\frac{1}{4}$ magnitude. At $10^h\ 17^m\ 48^s{\cdot}7$ G.M.T. a comparison with the star Argelander 178,8 = 190,112 made with the equatorially-mounted achromatic of 5 inches aperture, and a dark-field photographed micrometer constructed by Mr. Dancer, gave the comet's apparent place R.A. $10^h\ 5^m\ 27^s{\cdot}76$, Dec. $+48^\circ\ 52'\ 7''{\cdot}7$.

Turning the 13-inch reflector upon the comet with powers of 81 and 196, it was found that the nebulosity was more than 20′ in diameter, considerably condensed in the middle, but without any distinct planetary or stellar nucleus. There was a faint tapering elongation extending about a quarter of a degree from the north following side; and stars of the 11th and 12th magnitude were easily seen through the comet at the distance of half a radius from its centre.



COMET I. 1861,
As seen with Mr. Worthington's 13-inch Reflector, May 4th.

May 4th. Three comparisons with Argelander 173,122 gave the place of the comet at $9^h\ 26^m\ 19^s{\cdot}3$ G.M.T. R.A. $9^h\ 52^m\ 19^s{\cdot}83$, Dec. $+45°\ 18'\ 28''{\cdot}1$.

With the 13-inch reflector the diameter of the nebulosity constituting the head of the comet, carefully estimated by comparison with the known diameter of the field of view, was 22′. It was much condensed in the middle, but there was certainly no distinct stellar nucleus. The centre of greatest condensation was not in the centre of the nebulosity, but towards the *north following*

side. The tapering elongation of last night was now a narrow and slightly fan-shaped tail of $2\frac{1}{4}$ degrees in length, but apparently separated from the nebulosity of the head by a remarkable and comparatively dark interval, as shown in the sketch which accompanies this paper. The point of origin of this singular tail was estimated to be from 12 to 15 minutes distant from the centre of the head; and its breadth at this part was about $4\frac{1}{2}$ minutes, and at its extremity about 15 minutes. Its axis was perfectly straight; and its brightness was greatest at the narrow end, where it was equal to that of the nebulosity of the head at two-thirds of the radius from the centre.

May 5th. At $10^h\ 16^m\ 59^s{\cdot}7$ G.M.T., six comparisons with Lalande's 19,168 gave the comet's apparent place R.A. $9^h\ 39^m\ 37^s{\cdot}82$, Dec. $+41°\ 21'\ 34''{\cdot}5$.

The sky to-night was not very favourable for the observation of faint objects; but the general features of the comet did not appear to have undergone any material change. Last night it occurred to me, after leaving the observatory, that the axis of the tail was not exactly in the direction of the comet's radius vector, and to-night I found its angle of position at $13^h\ 35^m$ sidereal time to be $96°{\cdot}7$. At this time the position of the sun and comet were—

The sun. . . . R.A. $42°\ 54'$; N.P.D. $73°\ 33'$.
The comet. . R.A. $144°\ 50'$; N.P.D. $48°\ 43'$.

From these data we find that the angle of position of a prolongation of the comet's radius vector was $69°{\cdot}9$. The apparent deviation of the axis of the tail was therefore $26°{\cdot}8$ in the direction of the comet's motion.

May 7th. At 12^h G.M.T. the comet appeared to the naked eye to be nearly equal to μ Leonis, and equal to, if not brighter than, 38 Lyncis.

May 9th. Three comparisons with Lalande's 17,987

gave the comet's apparent position at $9^h\ 57^m\ 30^s{\cdot}5$, R.A. $9^h\ 3^m\ 23^s{\cdot}25$, Dec. $+26°\ 11'\ 26''{\cdot}1$.

At $13^h\ 15^m$ sid. time the angle of position of the axis of the tail was $103°{\cdot}2$. At this time the angle of position of the comet's radius vector was $74°{\cdot}5$; the deviation therefore amounted to $28°{\cdot}7$.

With the 5-inch achromatic the tail appeared to be half a degree in length; but with the 13-inch reflector it was fully one degree, though fainter than when last observed, and still much less in breadth than the diameter of the head. The average diameter of the head was about 20′; but the nebulosity extended further on the south preceding side of the point of greatest condensation than on the north preceding or north following sides. There was still an entire absence of any stellar nucleus. To the naked eye the comet appeared as a star about equal in brightness to μ Leonis.

May 14th. Notwithstanding the moonlight, the comet was still visible to the naked eye, and in the 5-inch achromatic with a power of 68 it was about 10′ in diameter. The tail, however, could not now be seen.

May 17th, $10^h\ 25^m$. The comet, though at a very low altitude and with strong moonlight, was still very easily seen with the 5-inch achromatic, and did not appear to have diminished since the 14th instant. This was the last opportunity I had of observing it.

Lalande's stars Nos. 19,168 and 17,987 occur in Bessel's Zones Nos. 454 and 347, and Bessel's places have been used in making the reductions.

II. *On the Irregular Barometric Oscillations at Geneva and on the Great St. Bernard, and their relations to the Mean Temperature and the fall of Rain.* By G. V. Vernon, Esq., F.R.A.S., M.B.M.S.

Read October 15th, 1861.

The present investigation has been undertaken in order to see exactly what effect great altitudes produce upon the irregular oscillations of the barometer. The whole of the observations used at the two stations were made under the direction of Prof. Plantamour of Geneva; and this is quite sufficient proof of their trustworthiness. The observatory of Geneva is situated in latitude 46° 11′ 59″ N., and is 1335 feet above the sea. The Hospice of the Great St. Bernard, at which the observations were made, is situated in latitude 45° 15′ 16″ N., and is 8173 feet above the sea. The distance between the two stations, measured upon a horizontal plane, is 58 miles approximately, and the difference of altitude 6838 feet.

The data have been reduced and tabulated in the same manner as in my paper upon similar oscillations at Manchester*; so that it will not be necessary to describe the process.

Table I. contains the amounts of oscillation and the number of oscillations for each station, arranged under the separate months.

Table II. contains the mean monthly temperatures at Geneva, and their differences from the mean of 20 years.

Table III. contains similar data for the Great St. Bernard.

Table IV. contains the fall of rain and snow at Geneva for each month, and the differences from 33 years' mean.

* Vol. I. (Third Series) of the Society's Memoirs.

Table V. contains similar data for the Great St. Bernard, and the differences from 20 years' mean.

Table VI. gives the relation between the mean temperature and the amount of the oscillations at the two stations.

Table VII. gives the relation between the number of the oscillations and the fall of rain at the two stations.

Table VIII. gives the relation between the fall of rain and the amount of the oscillations at the two stations.

The maximum amount of oscillation at Geneva occurs in January, and the minimum in August. The maximum amount at the Great St. Bernard occurs in December; and there appear to be two minima, one in June and the other in August.

The two curves (Plate I.) approach one another very closely in July and August, as the following Table indicates:—

Month.	Mean daily oscillation.			Mean monthly temperature.			Number of oscillations.	
	Geneva.	Gt. St. Bernard	Difference.	Geneva.	Gt. St. Bernard	Difference.	Geneva.	Gt. St. Bernard
	in.	in.	in.	°	°	°		
January ...	0·138	0·107	0·031	31·5	14·4	17·1	14·7	12·9
February ...	0·133	0·106	0·027	34·2	16·3	17·9	13·1	12·3
March	0·123	0·103	0·020	38·9	18·7	20·2	14·6	12·9
April	0·118	0·090	0·028	46·7	25·8	20·9	14·0	13·0
May	0·092	0·075	0·017	54·3	32·3	22·0	15·0	14·1
June.........	0·079	0·064	0·015	61·9	39·6	22·3	14·0	12·3
July.........	0·075	0·068	0·007	64·3	42·6	21·7	14·0	13·7
August......	0·071	0·064	0·007	63·1	42·1	21·0	16·0	14·2
September ..	0·085	0·068	0·017	57·0	37·4	19·6	13·9	13·7
October ..	0·115	0·088	0·027	49·1	30·7	18·4	14·1	13·1
November ..	0·125	0·091	0·034	40·5	22·0	18·5	13·1	11·9
December ..	0·129	0·111	0·018	33·0	18·5	14·5	14·8	13·1

Upon comparing the amount of oscillation with the mean temperature, it will be seen, generally, that the amount of oscillation diminishes as the mean temperature increases, and *vice versâ*. With a difference of 32°·8 between the warmest and coldest months at Geneva, we

have a difference in the amount of oscillation of 0·063 inch, whilst at the Great St. Bernard a difference of 28°·2 gives 0·039 inch as the difference in the amount of oscillation. The period of minimum oscillation appears to occur somewhat later than the period of maximum temperature.

As the stations become more elevated above the sea, the curve appears to become flattened, so that at some particular altitude, at present unknown, the curve would approach a straight line, and nearly all the disturbance would disappear, or, at least, owing to the greatly diminished density of the air, become imperceptible.

No law regulating the number of oscillations can be deduced from these observations. The maximum number of oscillations occurs at Geneva in August, and two minima in February and November; at the Great St. Bernard the maximum occurs in August, and a single minimum in November.

The mean daily amount of oscillation for the year is, at Geneva, 0·1069 inch, the total for the year being 39·0719 inches; at the Great St. Bernard the mean daily amount is 0·0865 inch, total for the year 31·5941 inches.

The mean annual number of oscillations is, for Geneva, 171·3; and for the Great St. Bernard 157·2.

	Geneva.	Great St. Bernard.
Number of oscillations in the six winter months	84·4	76·2
Number of oscillations in the six summer months	86·9	81·0

There appears to be a greater relative increase in the summer months at the Great St. Bernard than at Geneva: Geneva gives an increase of 2·96 per cent., whilst the Great St. Bernard gives 6·30 per cent.

The following small Table gives the total amount of oscillation for each year, and the total number of oscillations:—

	Geneva.		Great St. Bernard.	
	Amount of oscillation.	Number of oscillations.	Amount of oscillation.	Number of oscillations.
	inches.		inches.	
1848.	44·079	168	Incom	plete.
1849.	41·536	167	33·384	169
1850.	39·434	169	32·070	168
1851.	35·308	179	29·433	154
1852.	39·834	161	31·961	149
1853.	39·533	173	Incom	plete.
1854.	36·995	183	31·562	163
1855.	41·026	169	31·407	152
1856.	41·500	184	33·244	166
1857.	31·103	135	26·413	124

The maximum amount of oscillation appears to have been in 1848, and the minimum in 1857.

Separating the number of oscillations in each year, according as they are above or below the average, we find—

Geneva.

Oscillations compared with the average.	Corresponding amount of oscillation.
+ 7·77	38·966 inches.
– 10·65	39·138 „

showing that a number of oscillations above the average is accompanied by a less amount of oscillation than when the number of oscillations is below the average.

Great St. Bernard.

Oscillations compared with the average.	Corresponding amount of oscillation.
+ 10·90	32·565 inches.
– 10·85	29·804 „

that is, a number of oscillations above the average is accompanied by a larger amount of oscillation than when the number is below the average. This is a curious fact, as it is the direct converse of what takes place at Geneva. Can there be a point between the two stations at which the amounts of oscillation are the same, whether the number is above or below the average?

We now come to the effects of temperature above or below the average, as given in Table VI.

In January, February, and December, a temperature above the average at Geneva is accompanied by a greater amount of oscillation than when the temperature is below the average: in the months of March, April, May, June, July, August, September, October, and November, the opposite of the above holds good.

On the mean of the year, a temperature below the average at Geneva gives a larger amount of oscillation than a temperature above the average.

At the Great St. Bernard, temperatures above the average, in the months of August and September, give a larger amount of oscillation than temperatures below the average: the months of January, February, March, April, May, June, July, October, November, and December give the converse of the above.

For the year, a temperature below the average at the Great St. Bernard gives a larger amount of oscillation than a temperature above the average. The months of December, January, and February, at Geneva, appear to correspond, in their relations to the amount of oscillation, with the months of August and September at the Great St. Bernard.

A number of oscillations above the average at Geneva, in the months of January, March, June, July, August, and November, is accompanied by a larger rain-fall than when the number of oscillations is below the average. In the months of February, April, May, September, October, and December, the opposite of the above takes place.

Upon the mean of the year, a number of oscillations above the average is accompanied by a larger amount of rain-fall than when the number of oscillations is below the average.

At the Great St. Bernard, a number of oscillations

above the average, in the months of January, February, March, April, May, August, September, and November, is accompanied by a larger amount of rain-fall than a number of oscillations below the average. In the months of June, July, October, and December, the converse of this holds good.

On the mean of the year, a number of oscillations above the average at the Great St. Bernard is accompanied by a larger amount of rain-fall than when the number is below the average.

We now come to the effect of the rain-fall. A rain-fall above the average at Geneva, in every month of the year, is accompanied by a larger amount of oscillation than a rain-fall below the average.

At the Great St. Bernard, a fall of rain above the average, in the months of January, February, March, July, November, and December, is accompanied by a larger amount of oscillation than when the rain-fall is below the average. During the remaining months of the year the converse of this holds good.

Upon the mean of the year, a rain-fall below the average is accompanied by a less amount of oscillation than when the rain-fall is above the average: this agrees with what has been deduced for Geneva; but still, during some of the months, it appears as if some disturbing cause existed at the higher elevation which did not exist at the lower.

The mean readings of the barometer for each month, reduced to 32° F., during the period 1848–1858, were as follows:—

	Geneva.	Great St. Bernard.	Differences.
	inches.	inches.	inches.
January	28·607	22·057	6·550
February	28·619	22·074	6·545
March	28·566	22·044	6·522
April	28·497	22·077	6·420
May	28·528	22·161	6·367
June	28·623	22·338	6·285
July	28·638	22·377	6·261
August	28·642	22·381	6·261
September	28·652	22·335	6·317
October	28·593	22·268	6·325
November	28·576	22·087	6·489
December	28·701	22·156	6·545

Upon comparing these figures with the amounts of oscillation, we find that at Geneva the minimum pressure occurs in April, and the maximum in December, neither of which dates agrees exactly with that of maximum or minimum amount of oscillation.

At the Great St. Bernard, the minimum pressure occurs in March, or a month earlier than at Geneva, whilst the maximum occurs in August. The maximum pressure at this station occurs at the period of *minimum* oscillation, whilst at Geneva the period of maximum pressure (December) is a month earlier than the period of *maximum* oscillation.

On comparing the differences between the mean pressures at the two stations, it will be observed that this difference is at a maximum in January, gradually diminishes to July and August, and then increases to the end of the year. From the same data we find that the differences in the amounts of atmospheric pressure diminish as the differences between the mean temperatures of the two stations increase.

The fall of rain and snow at Geneva, on an average of 33 years, was 32·224 inches. Taking an average of 11 years, 126 days are rainy. At the Great St. Bernard, according to 20 years' observations, the fall is 56·929

inches; and on an average of 11 years, 117·7 days are rainy. The fall at Geneva is greatest in summer and autumn, but at the Great St. Bernard it is greatest in autumn and winter. The greatest monthly fall occurs in October at Geneva, and in January upon the Great St. Bernard,—the former being two months after the period of minimum oscillation of the barometer, and the latter a month later than the maximum period of oscillation.

The conclusions which may be drawn from this investigation are the following:—As we ascend in the atmosphere, the amplitude of the irregular diurnal oscillations of the barometer gradually diminishes, more especially in the winter months, the summer months having an amount of oscillation not differing much from that of less elevated stations in nearly the same geographical position. Excessive rain-fall at stations of moderate elevation appears to be accompanied by a larger amount of oscillation than when the rain-fall is below the average: this law appears to hold good in every month.

At more elevated stations, the same law appears to exist for the entire year; but many of the months appear to be subjected to some disturbing cause, and do not conform to this law. It remains to be seen whether a long series of years would eliminate this disturbance, or whether it may be owing to some other elements which, at the higher station, produce effects dissimilar to those produced at the lower station. Temperatures below the average of the season greatly increase the amount of disturbance.

Kämtz, in his 'Handbook of Meteorology,' gives the irregular oscillations for each month, and for various places; he, however, omits to state what particular years were used, and how many years he deduced each mean value from. The following small Table contains his values for Geneva and the Great St. Bernard:—

	Geneva.	Great St. Bernard.
	inch.	inch.
January	0·166	0·111
February	0·153	0·111
March	0·150	0·115
April	0·108	0·094
May	0·091	0·070
June	0·074	0·074
July	0·072	0·072
August	0·074	0·065
September	0·092	0·083
October	0·109	0·092
November	0·114	0·084
December	0·136	0·110
Mean for the year	0·1115	0·0901

The figures for Geneva give a maximum in January, and a minimum in July, instead of August; but it is probable he has used very few years, as his maxima and minima appear to be greatly in excess of my determinations, especially the former.

The Great-St.-Bernard values exhibit very great irregularities, giving a maximum in March, and a minimum in August: the period from April to December is very irregular, and does not conform to any regular progressive increase or decrease in the amount of oscillation. There can be little doubt that these values have been determined from very few years' observations.

I have given these values, in order to show what has been done previously on this point.

Note.—It may be as well to state that the means for the year, given in Tables VI., VII., and VIII., are not the means of the twelve monthly values immediately above, but have been determined by giving weight to each month according to the number of observations from which its values have been derived.

TABLE I.—Barometric Oscillations, from observations made at 9 A.M.

JANUARY.

	Geneva.			Great St. Bernard.		
Year.	Total amount of oscillation.	Mean daily amount.	Number.	Total amount of oscillation.	Mean daily amount.	Number.
	inches.	inch.		inches.	inch.	
1848.	3·641	0·117	10	Incom	plete.	
1849.	3·941	0·127	16	3·153	0·102	14
1850.	5·077	0·164	15	4·101	0·132	15
1851.	3·640	0·117	14	2·729	0·088	13
1852.	4·224	0·136	12	3·454	0·114	12
1853.	4·452	0·144	18	3·346	0·108	14
1854.	4·035	0·130	18	3·216	0·104	12
1855.	3·316	0·107	14	3·138	0·101	12
1856.	5·349	0·172	16	3·291	0·106	14
1857.	5·827	0·188	11	3·891	0·126	12
1858.	2·961	0·096	13	2·770	0·089	11
Means.	4·282	0·138	14·7	3·309	0·107	12·9

FEBRUARY.

1848.	5·725	0·197	12	4·286	0·147	13
1849.	2·947	0·105	13	2·560	0·091	16
1850.	3·984	0·142	12	3·758	0·134	14
1851.	3·152	0·112	16	2·389	0·085	13
1852.	4·471	0·154	14	3·550	0·122	14
1853.	4·727	0·168	10	4·061	0·145	10
1854.	4·049	0·144	14	3·463	0·123	12
1855.	4·595	0·164	13	3·073	0·110	11
1856.	3·386	0·117	15	2·318	0·079	13
1857.	1·519	0·054	12	1·280	0·045	9
1858.	2·945	0·105	13	2·501	0·089	11
Means.	3·773	0·133	13·1	3·022	0·106	12·3

MARCH.

1848.	4·760	0·154	16	3·868	0·124	15
1849.	5·026	0·162	14	4·350	0·140	12
1850.	4·008	0·129	12	3·337	0·107	12
1851.	3·744	0·121	19	2·689	0·087	13
1852.	3·865	0·124	12	3·573	0·115	12
1853.	3·566	0·115	14	3·022	0·097	13
1854.	2·218	0·071	17	1·845	0·059	17
1855.	4·799	0·155	13	3·304	0·106	13
1856.	3·011	0·097	14	2·297	0·074	14
1857.	3·132	0·101	13	2·888	0·093	7
1858.	4·003	0·130	17	3·877	0·125	14
Means.	3·830	0·123	14·6	3·186	0·103	12·9

Table I. (*continued*).

APRIL.

	Geneva.			Great St. Bernard.		
Year.	Total amount of oscillation.	Mean daily amount.	Number.	Total amount of oscillation.	Mean daily amount.	Number.
	inches.	inch.		inches.	inch.	
1848.	3·904	0·130	11	2·436	0·081	12
1849.	3·929	0·131	18	2·956	0·099	17
1850.	3·931	0·131	14	2·360	0·079	12
1851.	3·142	0·105	15	2·397	0·080	15
1852.	2·794	0·093	10	2·116	0·071	11
1853.	3·871	0·129	15	2·734	0·091	11
1854.	3·634	0·121	16	2·910	0·097	17
1855.	3·295	0·110	15	3·138	0·105	10
1856.	3·298	0·110	13	2·852	0·095	15
1857.	4·171	0·139	13	2·940	0·098	9
1858.	2·887	0·096	13	2·880	0·096	13
Means.	3·533	0·118	14·0	2·520	0·090	13·0

MAY.

1848.	2·372	0·076	15	1·713	0·055	12
1849.	2·722	0·088	14	1·668	0·054	16
1850.	3·108	0·100	14	1·938	0·062	14
1851.	2·587	0·083	17	2·111	0·068	16
1852.	2·315	0·074	14	1·863	0·060	10
1853.	3·298	0·106	14	2·919	0·094	14
1854.	1·902	0·061	17	1·941	0·062	15
1855.	3·906	0·126	13	2·520	0·081	13
1856.	3·296	0·106	16	2·822	0·091	14
1857.	2·341	0·075	13	1·987	0·064	14
1858.	3·515	0·113	18	3·974	0·128	17
Means.	2·851	0·092	15·0	2·314	0·075	14·1

JUNE.

1848.	2·656	0·088	17	1·987	0·066	17
1849.	2·513	0·084	13	1·938	0·065	8
1850.	2·323	0·074	19	1·448	0·048	13
1851.	2·042	0·068	9	1·664	0·055	13
1852.	1·754	0·058	14	1·657	0·055	13
1853.	2·498	0·083	12	2·397	0·080	10
1854.	2·892	0·096	15	1·920	0·064	10
1855.	3·233	0·108	16	2·098	0·070	14
1856.	2·858	0·095	14	2·629	0·088	14
1857.	2·276	0·076	11	2·309	0·077	9
1858.	1·285	0·043	14	1·194	0·040	15
Means.	2·393	0·079	14·0	1·931	0·064	12·3

TABLE I. (*continued*).

Year.	Geneva. Total amount of oscillation.	Geneva. Mean daily amount.	Geneva. Number.	Great St. Bernard. Total amount of oscillation.	Great St. Bernard. Mean daily amount.	Great St. Bernard. Number.
JULY.						
	inches.	inch.		inches.	inch.	
1848.	2·598	0·083	17	2·096	0·068	13
1849.	2·461	0·079	14	2·177	0·070	12
1850.	1·789	0·058	12	1·889	0·061	17
1851.	3·066	0·099	11	2·900	0·093	13
1852.	2·181	0·070	11	1·850	0·060	10
1853.	2·236	0·072	14	2·306	0·074	14
1854.	1·991	0·064	14	1·916	0·062	12
1855.	2·171	0·070	11	2·161	0·070	15
1856.	2·414	0·078	19	1·842	0·059	14
1857.	1·549	0·050	15	1·728	0·056	13
1858.	3·104	0·100	16	2·438	0·079	18
Means.	2·323	0·075	14·0	2·118	0·068	13·7
AUGUST.						
1848.	2·704	0·087	17	2·109	0·068	12
1849.	2·326	0·075	16	1·834	0·059	16
1850.	2·586	0·083	16	2·041	0·066	16
1851.	2·188	0·071	15	2·312	0·075	15
1852.	2·316	0·075	20	2·200	0·071	14
1853.	1·910	0·062	20	1·849	0·060	13
1854.	1·765	0·057	17	1·407	0·045	15
1855.	1·727	0·056	13	1·638	0·053	16
1856.	2·482	0·080	15	2·268	0·073	11
1857.	1·917	0·062	11	2·110	0·068	11
1858.	2·173	0·070	16	2·090	0·067	17
Means.	2·190	0·071	16·0	1·987	0·064	14·2
SEPTEMBER.						
1848.	2·733	0·091	11	1·930	0·064	14
1849.	2·876	0·096	14	1·979	0·066	16
1850.	2·162	0·072	14	1·795	0·060	16
1851.	2·036	0·068	18	1·981	0·066	15
1852.	2·896	0·097	11	1·808	0·060	14
1853.	2·821	0·094	15	2·328	0·078	10
1854.	2·006	0·067	15	1·811	0·060	13
1855.	2·732	0·091	15	2·584	0·086	13
1856.	3·490	0·116	14	2·430	0·081	12
1857.	2·233	0·074	12	2·054	0·068	16
1858.	2·006	0·067	14	1·750	0·058	11
Means.	2·544	0·085	13·9	2·041	0·068	13·7

TABLE I. (*continued*).

	Geneva.			Great St. Bernard.		
Year.	Total amount of oscillation.	Mean daily amount.	Number.	Total amount of oscillation.	Mean daily amount.	Number.
	inches.	inch.		inches.	inch.	
OCTOBER.						
1848.	3·992	0·129	15	3·005	0·097	18
1849.	4·330	0·140	11	3·612	0·116	14
1850.	3·766	0·121	14	3·695	0·119	12
1851.	3·190	0·103	9	2·793	0·090	7
1852.	3·625	0·117	14	2·331	0·075	15
1853.	3·661	0·118	11	2·633	0·085	11
1854.	4·156	0·134	18	2·833	0·091	16
1855.	3·639	0·117	13	2·509	0·081	10
1856.	1·981	0·064	21	1·715	0·055	18
1857.	3·632	0·117	15	2·867	0·093	13
1858.	3·223	0·104	14	2·126	0·069	10
Means.	3·563	0·115	14·1	2·738	0·088	13·1
NOVEMBER.						
1848.	5·440	0·181	14	3·700	0·123	12
1849.	4·059	0·136	6	3·909	0·130	12
1850.	3·728	0·124	13	3·230	0·108	12
1851.	4·184	0·139	17	3·237	0·108	11
1852.	4·727	0·158	14	3·383	0·113	13
1853.	2·915	0·097	17	....	....	......
1854.	3·257	0·109	10	2·934	0·098	10
1855.	2·824	0·094	15	1·820	0·061	10
1856.	4·032	0·134	14	3·079	0·103	15
1857.	2·506	0·084	9	2·359	0·079	11
1858.	3·467	0·116	15	2·462	0·082	13
Means.	3·740	0·125	13·1	2·738	0·091	11·9
DECEMBER.						
1848.	3·554	0·114	13	2·810	0·091	12
1849.	4·406	0·142	18	3·248	0·104	16
1850.	2·972*	0·096	14	2·478	0·080	15
1851.	2·337	0·075	19	2·231	0·072	10
1852.	4·666	0·150	15	4·176	0·134	11
1853.	3·578	0·115	13	3·022	0·097	13
1854.	5·090	0·164	12	5·366	0·173	14
1855.	4·789	0·154	18	3·224	0·104	15
1856.	5·903	0·190	13	5·701	0·184	12
1857.	2·772	0·089	13	2·351	0·076	13
1858.	....	......	......	....	....	......
Means.	4·007	0·129	14·8	3·461	0·111	13·1

Table II.—Monthly Temperatures at Geneva.

Year.	January. Temperature.	January. Difference from average of 20 years.	February. Temperature.	February. Difference from average of 20 years.
	°	°	°	°
1848.	24·5	−7·0	37·8	+3·6
1849.	35·3	+3·8	36·5	+2·3
1850.	27·5	−4·0	39·2	+5·0
1851.	33·3	+1·8	34·3	+0·1
1852.	36·1	+4·6	36·3	+2·1
1853.	37·7	+6·2	31·5	−2·7
1854.	31·8	+0·3	30·1	−4·1
1855.	29·2	−2·3	35·2	+1·0
1856.	36·3	+4·8	37·8	+3·6
1857.	32·4	+0·9	31·6	−2·6
1858.	27·5	−4·0	33·0	−1·2
Mean of 20 years.	31·5	......	34·2	......

Year.	March. Temperature.	March. Difference from average of 20 years.	April. Temperature.	April. Difference from average of 20 years.
	°	°	°	°
1848.	39·9	+1·0	49·2	+2·5
1849.	37·8	−1·1	42·8	−3·9
1850.	36·3	−2·6	46·5	−0·2
1851.	38·7	−0·2	48·4	+1·7
1852.	36·7	−2·2	46·3	−0·4
1853.	32·8	−6·1	45·2	−1·5
1854.	40·1	+1·2	49·5	+2·8
1855.	40·3	+1·4	46·2	−0·5
1856.	40·4	+1·5	49·8	+3·1
1857	39·4	+0·5	45·3	−1·4
1858.	38·8	−0·1	51·8	+5·1
Mean of 20 years.	38·9	......	46·7	......

Year.	May. Temperature.	May. Difference from average of 20 years.	June. Temperature.	June. Difference from average of 20 years.
	°	°	°	°
1848.	56·7	+2·4	61·1	−0·8
1849.	55·2	+0·9	64·5	+2·6
1850.	51·6	−2·7	62·2	+0·3
1851.	50·0	−4·3	63·5	+1·6
1852.	55·3	+1·0	59·7	−2·2
1853.	52·5	−1·8	60·1	−1·8
1854.	55·4	+1·1	60·1	−1·8
1855.	52·0	−2·3	60·3	−1·6
1856.	51·6	−2·7	62·2	+0·3
1857.	54·9	+0·6	61·0	−0·9
1858.	52·3	−2·0	66·4	+4·5
Mean of 20 years.	54·3	......	61·9	......

TABLE II. (*continued*).

Year.	July. Temperature.	July. Difference from average of 20 years.	August. Temperature.	August. Difference from average of 20 years.
	°	°	°	°
1848.	64·8	+0·5	63·2	+0·1
1849.	65·5	+1·2	61·7	−1·4
1850.	63·9	−0·4	62·5	−0·6
1851.	62·1	−2·2	63·1	0·0
1852.	66·4	+2·1	61·9	−1·2
1853.	65·1	+0·8	64·8	+1·7
1854.	64·5	+0·2	61·9	−1·2
1855.	63·9	−0·4	66·3	+3·2
1856.	64·1	−0·2	67·9	+4·8
1857.	68·8	+4·5	64·8	+1·7
1858.	62·4	−1·9	60·9	−2·2
Mean of 20 years.	64·3	......	63·1	......

Year.	September. Temperature.	September. Difference from average of 20 years.	October. Temperature.	October. Difference from average of 20 years.
	°	°	°	°
1848.	56·5	−0·5	49·0	−0·1
1849.	58·3	+1·3	51·0	+1·9
1850.	54·8	−2·2	45·5	−3·6
1851.	52·2	−4·8	49·3	+0·2
1852.	56·9	−0·1	48·4	−0·7
1853.	56·6	−0·4	49·5	+0·4
1854.	58·7	+1·7	50·5	+1·4
1855.	59·5	+2·5	52·3	+3·2
1856.	55·9	−1·1	50·0	+0·9
1857.	60·8	+3·8	51·3	+2·2
1858.	60·8	+3·8	50·7	+1·6
Mean of 20 years.	57·0	......	49·1	......

Year.	November. Temperature.	November. Difference from average of 20 years.	December. Temperature.	December. Difference from average of 20 years.
	°	°	°	°
1848.	36·9	−3·6	33·4	+0·4
1849.	36·8	−3·7	31·6	−1·4
1850.	42·7	+2·2	34·5	+1·5
1851.	32·5	−8·0	25·9	−7·1
1852.	45·3	+4·8	37·9	+4·9
1853.	41·7	+1·2	28·8	−4·2
1854.	38·0	−2·5	36·6	+3·6
1855.	39·3	−1·2	27·0	−6·0
1856.	36·0	−4·5	33·7	+0·7
1857.	41·0	+0·5	33·0	0·0
1858.	37·3	−3·2	......	......
Mean of 20 years.	40·5	......	33·0	......

TABLE III.—Monthly Temperature at the Gt. St. Bernard.

Year.	January. Temperature.	January. Difference from average of 20 years.	February. Temperature.	February. Difference from average of 20 years.
	°	°	°	°
1848.	8·4	−6·0	19·4	+3·1
1849.	17·8	+3·4	21·7	+5·4
1850.	12·2	−2·2	21·9	+5·6
1851.	18·1	+3·7	16·4	+0·1
1852.	19·4	+5·0	14·8	−1·5
1853.	17·8	+3·4	6·6	−9·7
1854.	18·0	+3·6	10·8	−5·5
1855.	13·1	−1·3	17·9	+1·6
1856.	18·3	+3·9	23·1	+6·8
1857.	10·8	−3·6	16·5	+0·2
1858.	12·0	−2·4	14·0	−2·3
Average of 20 years.	14·4	......	16·3	......
	March.		April.	
	°	°	°	°
1848.	17·8	−0·9	28·9	+3·1
1849.	18·2	−0·5	20·8	−5·0
1850.	17·2	−1·5	25·3	−0·5
1851.	16·4	−2·3	26·9	+1·1
1852.	18·3	−0·4	24·9	−0·9
1853.	11·8	−6·9	21·7	−4·1
1854.	21·4	+2·7	27·5	+1·7
1855.	17·0	−1·7	24·7	−1·1
1856.	22·2	+3·5	26·7	+0·9
1857.	21·0	+2·3	22·9	−2·9
1858.	18·1	−0·6	29·8	+4·0
Average of 20 years.	18·7	......	25·8	......
	May.		June.	
	°	°	°	°
1848.	36·9	+4·6	40·6	+1·0
1849.	34·5	+2·2	43·3	+3·7
1850.	29·2	−3·1	40·6	+1·0
1851.	29·0	−3·3	40·6	+1·0
1852.	31·8	−0·5	36·9	−2·7
1853.	30·9	−1·4	37·7	−1·9
1854.	32·4	+0·1	37·2	−2·4
1855.	28·4	−3·9	37·0	−2·6
1856.	28·9	−4·4	39·6	0·0
1857.	33·0	+0·7	37·8	−1·8
1858.	29·8	−2·5	43·6	+4·0
Average of 20 years.	32·3	......	39·6	......

TABLE III. (*continued*).

Year.	July. Temperature.	July. Difference from average of 20 years.	August. Temperature.	August. Difference from average of 20 years.
	°	°	°	°
1848.	44·6	+2·0	44·2	+2·1
1849.	43·7	+1·1	41·4	−0·7
1850.	41·7	−0·9	41·0	−1·1
1851.	40·3	−2·3	42·8	+0·7
1852.	44·1	+1·5	40·6	−1·5
1853.	44·1	+1·5	45·1	+3·0
1854.	42·8	+0·2	41·5	−0·6
1855.	41·9	−0·7	44·5	+2·4
1856.	41·3	−1·3	46·1	+4·0
1857.	45·3	+2·7	42·7	+0·6
1858.	39·1	−3·5	39·1	−3·0
Average of 20 years.	42·6	......	42·1	......

Year.	September. Temperature.	September. Difference.	October. Temperature.	October. Difference.
	°	°	°	°
1848.	36·7	−0·7	29·1	−1·6
1849.	36·9	−0·5	33·3	+2·6
1850.	32·5	−4·9	25·3	−5·4
1851.	32·8	−4·6	33·0	+2·3
1852.	35·6	−1·8	30·6	−0·1
1853.	37·4	0·0	29·9	−0·8
1854.	40·8	+3·4	31·2	+0·5
1855.	38·9	+1·5	33·3	+2·6
1856.	32·8	−4·6	33·6	+2·9
1857.	39·7	+2·3	31·5	+0·8
1858.	40·3	+2·9	31·3	+0·6
Average of 20 years.	37·4	......	30·7	......

Year.	November. Temperature.	November. Difference.	December. Temperature.	December. Difference.
	°	°	°	°
1848.	17·7	− 4·3	22·0	+3·5
1849.	21·9	− 0·1	12·8	−5·7
1850.	25·2	+ 3·2	21·2	+2·7
1851.	9·6	−12·4	19·6	+1·1
1852.	28·8	+ 6·8	25·3	+6·8
1853.	23·9	+ 1·9	12·3	−6·2
1854.	17·5	− 4·5	15·1	−3·4
1855.	21·2	− 0·8	11·6	−6·9
1856.	17·0	− 5·0	17·2	−1·3
1857.	26·1	+ 4·1	23·8	+5·3
1858.	19·4	− 2·6	......	......
Average of 20 years.	22·0	......	18·5	......

Table IV.—Rain and Snow at Geneva.

Year.	January.		February.	
	Fall in inches.	Difference from mean.	Fall in inches.	Difference from mean.
1848.	0·858	−1·010	1·435	+0·115
1849.	3·303	+1·435	1·120	−0·200
1850.	1·634	−0·234	0·925	−0·395
1851.	2·020	+0·152	1·031	−0·289
1852.	1·803	−0·065	0·776	−0·544
1853.	2·354	+0·486	0·799	−0·521
1854.	0·843	−1·025	0·291	−1·029
1855.	1·354	−0·514	5·614	+4·294
1856.	4·968	+3·100	1·094	−0·226
1857.	1·236	−0·632	0·705	−0·615
1858.	0·181	−1·687	0·736	−0·584
Means.	1·868	......	1·320	......
	March.		April.	
1848.	3·465	+0·114	6·012	+3·464
1849.	1·088	−0·263	2·425	−0·123
1850.	0·185	−1·166	5·070	+2·522
1851.	2·831	+1·480	2·280	−0·268
1852.	0·299	−1·052	0·378	−2·170
1853.	0·654	−0·697	2·445	−0·103
1854.	0·055	−1·296	0·846	−1·702
1855.	1·713	+0·362	0·815	−1·733
1856.	2·457	+1·106	3·579	+1·031
1857.	1·040	−0·311	1·724	−0·824
1858.	1·079	−0·272	2·453	−0·095
Means.	1·351	......	2·548	......
	May.		June.	
1848.	0·772	−2·955	6·283	+3·112
1849.	3·177	−0·550	5·937	+2·766
1850.	4·445	+0·718	2·614	−0·557
1851.	1·949	−1·778	0·124	−3·047
1852.	2·248	−1·479	3·913	+0·742
1853.	5·953	+2·226	2·811	−0·360
1854.	2·425	−1·302	4·929	+1·758
1855.	2·882	−0·845	2·713	−0·458
1856.	11·724	+7·997	2·890	−0·281
1857.	2·157	−1·570	2·000	−1·171
1858.	3·264	−0·463	0·665	−2·506
Means.	3·727	......	3·171	......

TABLE IV. (*continued*).

Year.	July. Fall in inches.	July. Difference from mean.	August. Fall in inches.	August. Difference from mean.
1848.	2·776	−0·115	3·468	+0·282
1849.	1·898	−0·993	0·980	−2·206
1850.	0·142	−2·749	3·374	+0·188
1851.	5·319	+2·428	3·780	+0·594
1852.	2·335	−0·556	8·437	+5·251
1853.	3·343	+0·452	2·516	−0·670
1854.	4·039	+1·148	2·823	−0·363
1855.	2·980	+0·089	0·224	−2·962
1856.	2·728	−0·163	2·362	−0·824
1857.	0·734	−2·157	3·543	+0·357
1858.	5·504	+2·613	3·539	+0·353
Means.	2·891	......	3·186	......

Year.	September. Fall in inches.	September. Difference from mean.	October. Fall in inches.	October. Difference from mean.
1848.	2·087	−1·396	3·398	−1·280
1849.	3·670	+0·187	7·744	+3·066
1850.	3·075	−0·408	2·378	−2·300
1851.	3·382	−0·101	4·122	−0·556
1852.	7·343	+3·860	6·512	+1·834
1853.	4·571	+1·088	5·181	+0·503
1854.	0·000	−3·483	4·236	−0·442
1855.	4·346	+0·863	10·957	+6·279
1856.	4·598	+1·115	0·823	−3·855
1857.	2·397	−1·086	3·228	−1·450
1858.	2·843	−0·640	2·882	−1·796
Means.	3·483	......	4·678	......

Year.	November. Fall in inches.	November. Difference from mean.	December. Fall in inches.	December. Difference from mean.
1848.	1·657	−0·668	1·272	+0·088
1849.	1·724	−0·601	0·909	−0·275
1850.	2·992	+0·667	0·933	−0·251
1851.	1·256	−1·069	0·197	−0·987
1852.	5·039	+2·714	1·539	+0·355
1853.	1·405	−0·920	0·504	−0·680
1854.	3·098	+0·773	2·043	+0·859
1855.	2·453	+0·128	1·240	+0·056
1856.	1·220	−1·105	2·463	+1·279
1857.	1·622	−0·703	0·738	−0·446
1858.	3·110	+0·785	......	....
Means.	2·325	......	1·184	......

Table V.—Rain and Snow at the Great St. Bernard.

Year.	January. Fall in inches.	January. Difference from mean.	February. Fall in inches.	February. Difference from mean.
1848.	3·398	−0·760	10·929	+7·266
1849.	8·622	+4·464	2·441	−1·222
1850.	5·733	+1·575	5·189	+1·526
1851.	3·819	−0·339	3·350	−0·313
1852.	4·244	+0·086	4·961	+1·298
1853.	6·283	+2·125	5·795	+2·132
1854.	3·854	−0·304	0·961	−2·702
1855.	2·563	−1·595	3·531	−0·132
1856.	5·500	+1·342	1·701	−1·962
1857.	1·047	−3·111	0·000	−3·663
1858.	0·641	−3·517	1·441	−2·222
Means.	4·158	......	3·663	......
	March.		**April.**	
1848.	6·405	+3·373	11·008	+5·509
1849.	4·004	+0·972	8·591	+3·092
1850.	1·516	−1·516	5·249	−0·250
1851.	5·862	+2·830	8·756	+3·257
1852.	1·854	−1·178	3·248	−2·251
1853.	6·760	+3·728	7·024	+1·525
1854.	0·303	−2·729	1·992	−3·507
1855.	3·193	+0·161	3·390	−2·109
1856.	0·657	−2·375	5·421	−0·078
1857.	0·886	−2·146	3·780	−1·719
1858.	1·913	−1·119	2·039	−3·460
Means.	3·032	......	5·499	......
	May.		**June.**	
1848.	0·240	−4·749	8·858	+4·546
1849.	6·827	+1·838	7·803	+3·491
1850.	4·890	−0·099	2·626	−1·686
1851.	6·342	+1·353	0·921	−3·391
1852.	4·406	−0·583	4·878	+0·566
1853.	6·468	+1·479	3·268	−1·044
1854.	5·374	+0·385	6·760	+2·448
1855.	4·626	−0·363	4·953	+0·641
1856.	11·287	+6·298	3·303	−1·009
1857.	1·913	−3·076	2·953	−1·359
1858.	2·508	−2·481	1·110	−3·202
Means.	4·989	......	4·312	......

TABLE V. (*continued*).

Year.	July.		August.	
	Fall in inches.	Difference from mean.	Fall in inches.	Difference from mean.
1848.	3·638	+0·502	1·953	−1·141
1849.	3·173	+0·037	1·650	−1·444
1850.	1·425	−1·711	4·280	+1·186
1851.	8·484	+5·348	3·906	+0·812
1852.	2·169	−0·967	4·665	+1·571
1853.	1·831	−1·305	3·232	+0·138
1854.	5·189	+2·053	4·764	+1·670
1855.	2·248	−0·888	1·819	−1·275
1856.	2·535	−0·601	2·252	−0·842
1857.	0·028	−3·108	3·185	+0·091
1858.	3·772	+0·636	2·327	−0·767
Means.	3·136	......	3·094	......

	September.		October.	
1848.	2·728	+0·007	5·614	+0·848
1849.	2·839	+0·118	1·268	−3·498
1850.	1·382	−1·339	4·272	−0·494
1851.	5·728	+3·007	3·327	−1·439
1852.	4·350	+1·629	5·150	+0·384
1853.	2·713	−0·008	6·783	+2·017
1854.	0·031	−2·690	4·161	−0·605
1855.	2·480	−0·241	12·524	+7·758
1856.	3·555	+0·834	3·339	−1·427
1857.	2·398	−0·323	5·228	+0·462
1858.	1·728	−0·993	0·764	−4·002
Means.	2·721	......	4·766	......

	November.		December.	
1848.	4·031	+0·590	3·583	+0·954
1849.	5·748	+2·307	4·555	+1·926
1850.	5·551	+2·110	1·925	−0·704
1851.	3·106	−0·335	0·106	−2·523
1852.	1·827	−1·614	6·007	+3·378
1853.	2·980	−0·461	2·504	−0·125
1854.	4·551	+1·110	2·350	−0·279
1855.	3·437	−0·004	1·413	−1·216
1856.	0·996	−2·445	3·520	+0·891
1857.	1·165	−2·276	0·331	−2·298
1858.	4·461	+1·020	......	....
Means.	3·441	......	2·629	......

Table VI.—Temperature above or below the average compared with the corresponding amount of Oscillation.

Geneva.

Month.	Temperature above the the average.	Amount of oscillation.	Temperature below the average.	Amount of oscillation.
	°	inches.	°	inches.
January	+3·20	4·496	−3·43	3·749
February	2·53	4·037	2·65	3·310
March............	1·12	3·584	2·05	4·035
April	3·38	3·431	2·31	3·590
May	1·20	2·330	2·63	3·285
June	1·86	2·204	1·51	2·552
July	1·55	2·169	1·02	2·509
August	2·30	2·148	1·32	2·233
September	2·62	2·371	1·52	2·689
October	1·47	3·476	1·47	3·794
November	2·18	3·469	3·81	3·895
December	+2·22	4·437	−4·67	3·778
Year.	+2·12	3·235	−2·42	3·380

Great St. Bernard.

Month.	Temperature above the the average.	Amount of oscillation.	Temperature below the average.	Amount of oscillation.
	°	inches.	°	inches.
January	+3·83	3·198	−3·10	3·475
February	3·26	2·809	4·75	3·394
March............	2·87	2·343	1·85	3·503
April	2·16	2·695	2·42	2·707
May	1·90	1·827	2·73	2·592
June	2·14	1·646	2·28	2·076
July	1·50	2·012	1·74	2·246
August	2·13	2·048	1·38	1·914
September	2·52	2·049	2·85	1·987
October	1·77	2·636	1·98	2·916
November	4·00	2·991	4·24	3·020
December	+3·88	2·809	−4·70	4·112
Year.	+2·53	2·445	−2·73	2·835

Table VII.—Number of Oscillations above or below the average, compared with the fall of Rain.

Geneva.

Month.	Oscillations above the average.	Correspond-ing fall of rain.	Oscillations below the average.	Correspond-ing fall of rain.
		inches.		inches.
January	+1·96	2·620	−2·37	1·241
February	1·65	0·798	0·96	1·619
March............	2·65	1·857	1·46	1·059
April	1·50	2·313	1·67	3·202
May	1·60	2·242	1·14	3·227
June	1·57	3·335	2·00	2·620
July	1·57	3·003	1·57	2·865
August	1·43	3·591	1·43	2·543
September	0·97	3·310	2·56	3·976
October	3·15	2·914	1·81	5·682
November	2·04	4·839	3·60	4·396
December	+2·70	0·971	−1·80	1·326
Year.	+1·81	2·856	−1·75	2·721

Great St. Bernard.

Month.	Oscillations above the average.	Correspond-ing fall of rain.	Oscillations below the average.	Correspond-ing fall of rain.
		inches.		inches.
January	+1·10	4·743	−1·10	3·726
February	1·53	4·753	1·70	2·346
March............	1·24	3·585	2·15	2·065
April	2·40	5·359	1·86	5·104
May	1·90	5·263	1·10	4·834
June	2·03	4·441	3·05	5·196
July	1·90	2·362	1·53	3·780
August	1·63	3·124	2·00	3·058
September	1·47	3·237	1·90	2·101
October	3·10	3·906	2·60	5·483
November	0·93	3·769	1·40	2·565
December	+1·90	2·561	−1·27	2·675
Year.	+1·56	3·911	−1·76	3·697

Table VIII.—Fall of Rain above or below the average, compared with the corresponding amount of Oscillation.

Geneva.

Month.	Rain above the average.	Correspond-ing amount of oscillation.	Rain below the average.	Correspond-ing amount of oscillation.
	inches.	inches.	inches.	inches.
January	+1·293	4·345	−0·738	4·154
February	2·204	5·160	0·497	3·464
March	0·765	4·078	0·722	3·688
April	2·339	3·711	0·877	3·465
May	3·647	3·234	1·367	2·707
June	2·095	2·454	1·197	2·359
July	1·346	2·514	1·122	2·165
August	1·171	2·314	1·405	2·042
September	1·423	2·963	1·186	2·196
October	2·920	3·814	1·668	3·420
November	1·014	3·601	0·844	3·856
December	+0·528	4·800	−0·528	3·213
Year.	+1·585	3·464	−1·002	3·105

Great St. Bernard.

Month.	Rain above the average.	Correspond-ing amount of oscillation.	Rain below the average.	Correspond-ing amount of oscillation.
	inches.	inches.	inches.	inches.
January	+1·918	3·469	−1·754	3·149
February	3·056	3·913	1·745	2·512
March	2·213	3·447	1·844	2·969
April	3·351	2·631	1·910	2·742
May	2·271	2·292	1·892	2·333
June	2·398	1·920	2·338	1·940
July	1·715	2·309	1·429	1·962
August	0·911	1·986	1·094	1·988
September	1·119	2·026	0·932	2·054
October	2·294	2·669	1·911	2·796
November	1·428	3·247	1·335	2·778
December	+1·787	3·984	−1·191	3·112
Year.	+1·977	2·774	−1·597	2·526

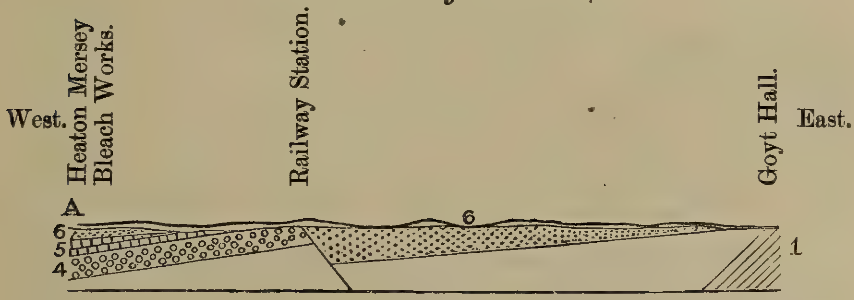
III. *Additional Observations on the Permian Beds of South Lancashire.*

By E. W. Binney, V.P., F.R.S., F.G.S.

Read November 26th, 1861.

In two previous papers* communicated to the Society, and printed in its Memoirs, descriptions have been given of some of the permian beds of the North-west of England. Since that time more information has come into my possession; and it is the object of this communication to bring it before the Society. The places where new sections have been met with are at Heaton Norris near Stockport, Medlock Vale between Manchester and Ashton, Chorlton-on-Medlock, and Ordsal near Manchester, and Skillaw Clough and Bentley Brook near Newburgh, between Wigan and Southport.

Heaton Mersey Section †.



Section from Heaton Mersey to Goyt Hall. Distance about four miles.

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* Vols. xii. and xiv. (Second Series) of the Society's Memoirs.

† In this, and the following sections illustrating the present memoir, the references will be as follows:—

6. Upper new red sandstone (trias).
5. Red marls, limestones, and conglomerate (permian).
4. Lower new red sandstone (permian).
3. Red clays.
2. Upper coal-measures.
1. Middle coal-measures.

1′. Lower coal-measures.

In my first paper, before alluded to, at p. 217 a short notice is given of a very interesting boring made some years ago at the bleach-works of Mr. Tait at Heaton Mersey, which proved that the permian beds there were of great thickness. Since that time, the lower new red sandstone has been exposed by Mr. Howard near the railway station at Heaton Norris, for the purpose of being quarried for moulding sand, like that at Collyhurst near Manchester; and the permian marls have been seen lying upon it, and succeeded in their turn by the beach beds of Mr. Hull, and then the pebble-beds of the trias up to Heaton Mersey Mill.

In my former paper, there was given the section of the bore at the last-named place, which was as follows:—

	feet.
Sand and gravel a few feet.	
Trias	45
Red and variegated beds of marl containing limestones	129
Lower new red sandstone (Collyhurst), proved	402
	576

This section is shown on the banks of the Mersey. Near the position of the bore marked A in the woodcut, and for about 300 yards towards Stockport, the trias cannot be seen, owing to the covering of valley-gravel. When it makes its appearance, it is as a bright-red and fine-grained sandstone containing pebbles; then come beds of red sandstone, which Mr. Hull considers to be like his pebble-beds. These are succeeded by some singular coarse beds containing angular pieces of quartz, which the same gentleman took to be old beach-beds. They dip to the S.W., at an angle of 12°.

Next appear red and variegated marls, which are exposed in the vacant piece of ground behind Well Lane, opposite Orrell's Mill. Twenty feet in thickness are seen; and they dip to the S.W., at an angle of 25°. Although examined with considerable care, no fossil shells were found

in them by me. These marls are succeeded by the lower new red sandstone, similar to that of Collyhurst, except that the conglomerate-bed was not exposed, and a small parting of marl occurred in the upper portion of the sandstone. On following the rock towards the London and North-Western Railway at Heaton Norris, it is seen much dislocated, and then cut off by a fault *, running south-east and north-west, which brings in the trias beds that underlie the town of Stockport. The fault inclines at an angle of 45°, to the north-east; and a thin bed of red clay appears to lie in it.

The trias beds, apparently the pebble-beds, are seen in Hatton Street dipping to the W.N.W., at an angle of 12°, and may be traced through Portwood and Newbridge along the valley of the Goyt, to the place near where the Fog brook joins the Goyt. The lowest beds of trias are of a deep red colour, soft, and comparatively free from pebbles †. They dip at an angle of 15°, to the S.S.W., and repose on beds of the lower part of the middle coalfield dipping to the S.S.W. at an angle of 80°. The trias beds, if they continue all this distance without any fault, from the Heaton Norris Railway Station to Goyt Hall, must be of great thickness; but most probably they may be repeated by one or more faults, although these are difficult to make out.

The fault at Heaton Norris, when taken in connexion with the occurrence of coal-measures in Chorlton-on-Medlock, hereinafter described, is of considerable interest. In my former communication, it was stated that the dis-

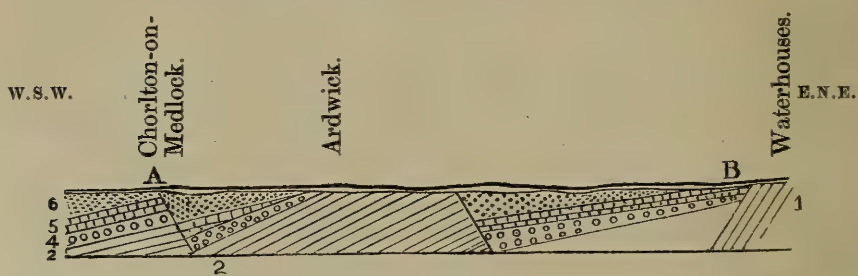
* This fault was first shown to me by Mr. E. Hull, B.A., F.G.S., of the Geological Survey.

† Mr. Hull is inclined to consider this sandstone as the lower new red sandstone (Collyhurst), and not trias. It is no doubt, as here stated, free from pebbles, and much like the last-named rock in its characters; but up to this time, to my knowledge, there is no other evidence to show that it is a permian, and not a trias rock.

covery of permian beds at Heaton Mersey was probably owing to the fault of Bradford and Clayton; but on carefully taking the direction of the fault at Heaton Norris, it appears to run more in a line through Chorlton-on-Medlock into the great Pendleton fault in the valley of the Irwell, than that of Bradford and Clayton.

It is much to be desired that a bore should be put down in Mr. Howard's sand-delf at Heaton Norris, to prove, first, the thickness of the permian beds there, and next the position of the coal-field lying under them. This would not cost much, and it would prove whether or not an extensive coal-field lies under Heaton Norris and Stockport and extends all the way to Manchester.

Manchester Section.—Medlock Vale.



Section from All Saints to Waterhouses. Distance about five miles.

Mr. John Wood, of Bank Bridge print-works, has done much towards proving the ground lying between his estate and Waterhouses. In my former papers it has been my duty to acknowledge his kindness in furnishing me with information; and I have again the pleasure to acknowledge his assistance.

In Mr. Wood's first attempt to find coal under the trias and permian deposits at Medlock Vale, he did not succeed in perforating the latter strata; but in a late endeavour he has been more successful, and gone through the permian beds into the coal-measures. What coal-measures these are has not yet been ascertained, but a complete section of the permian strata of that locality has been obtained.

The bore was made at the place marked B in the section, and the following strata were met with :—

	yds.	ft.	in.	
Soil	1	0	0	Drift.
Sand	1	0	0	
Gravel	3	1	0	
Stiff clay	1	1	0	
Gravel	2	0	6	
Red sandstone	7	2	0	Trias
Red marl	2	2	6	Permian marls with limestones and gypsum.
Grey band (like fullers'-earth)	0	0	2	
Red marl	6	1	10	
Grey stone	0	0	4	
Red marl	1	1	8	
Chalk band (granular gypsum)	0	0	2	
Red marl	3	2	10	
Grey rock	0	1	0	
Red marl	14	0	0	
Grey rock	0	2	0	
Red marl	1	1	2	
Grey rock	0	0	10	
Red marl	4	1	0	
Grey rock	0	2	0	
Red marl	10	1	7	
Grey band	0	0	2	
Red marl	7	2	2	
Grey band	0	0	2	
Red rock	0	2	8	
Hard grey rock	0	0	4	
Red marl	1	2	2	
Red rock	0	0	11	
Blue marl	0	0	4	
Red marl	1	0	5	
Blue marl	0	0	2	
Red marl	1	0	7	
Red rock	0	0	2	
Red marl	2	1	8	
Chalk band (granular gypsum)	0	0	3	
Red marl	0	2	2	
Chalk band (granular gypsum)	0	0	2	
Red marl	3	0	8	
Shaly blue marl	0	0	6	
Chalk band (granular gypsum)	0	0	2	
Red marl	0	2	0	
Carried forward	84	2	5	

	yds.	ft.	in.	
Brought forward	84	2	5	
Red and grey marl	0	1	4	Permian marls with limestones and gypsum.
Shaly red rock	0	1	0	
Red and blue marl	1	0	7	
Shaly blue marl	0	2	4	
Red marl	1	0	10	
Sandy red marl	1	0	9	
Red marl	5	2	11	
White rock (granular gypsum)	0	0	4	
Red marl	0	2	0	
Red and brown marl	0	1	6	
Grey sandstone	0	0	2	
Red marl	0	2	10	
Red sandstone	54	2	7	Permian lower new red sandstone.
Red marl	0	2	0	
Fine red sandstone, alternating with red marl	42	0	0	
Coarse red sandstone	15	1	8	
Fine red sandstone	14	1	3	
Grey sandstone	0	1	0	
Fine red sandstone	5	2	0	
Grey sandstone	0	1	10	
Fine red sandstone	7	0	1	
*Red marl	0	0	5	
Blue marl	0	0	5	Coal-measures.
Red and grey marl	0	1	10	
Dark red rock	0	2	0	
Red and grey marl	0	2	1	
Grey and red marl	0	0	10	
Red rock	0	0	4	
Black and grey rock	0	0	9	
Grey soapstone	1	0	5	
Red and grey soapstone	1	0	9	
Red rock	0	0	11	
Grey rock	0	1	9	
Red and grey rock	0	2	1	
Red and grey rock	1	0	6	
Red and grey rock	1	1	3	
Red and grey rock	1	0	9	
Dark grey and red rock	0	1	5	
Grey rock	2	0	9	
Red rock	0	0	2	
Grey rock	3	0	3	
Grey soapstone	0	1	11	
Black stone	0	1	2	
Carried forward	257	0	2	

	yds.	ft.	in.	
Brought forward	257	0	2	
Floor clay	1	1	10	Coal-measures.
Grey metal	0	1	4	
Blue metal and grey	3	0	11	
Dark metal	0	0	5	
Grey metal	0	2	5	
Red and grey metal	0	2	0	
Red rock	2	1	2	
Red and grey rock	1	1	3	
Grey rock, with small bands of red	2	0	9	
Hard rock	0	1	1	
Grey soapstone	0	0	4	
Rock bind	0	0	1	
Dark soapstone	1	0	1	
Grey soapstone	0	1	11	
Rock bind	0	0	2	
Grey soapstone	0	1	5	
Black stone containing a small seam of coal	0	0	9	
Grey soapstone	0	1	8	
Rock bind	0	0	1	
Grey soapstone	0	2	11	
Darker soapstone	0	2	6	
Black stone	0	0	2	
Floor clay	1	1	5	
Grey soapstone	3	1	11	
Rock bind	0	0	2	
Grey metal	1	0	4	
Very hard rock (light colour)	1	0	4	
Grey metal	0	2	11	
Fine clay	0	0	4	
Grey soapstone, with thin bands of light-coloured rock	2	2	4	
Reddish metal	0	1	4	
Light-coloured metal	0	0	11	
Total	287	1	5	

The trias in this section had nearly all outcropped; but the permian marls, containing numerous beds of limestone and fine beds of white granular gypsum, were 245 feet in thickness, the greatest thickness which they have yet been found in East Lancashire. In the late Mr. Bradbury's section, given by me in my former paper *, a query was

* Twelfth volume (Second Series) of the Memoirs of the Society, p. 222.

put as to the occurrence of the gypsum; but the present section clearly shows five beds, all fully as white and granular as the fine thick deposit met with in the permian marls at Barrow Mouth, and described by me at p. 254 of my first paper.

No notice of the conglomerate-bed is given in the bore; but it is probable that one was met with, as the pebbles usually characteristic of that deposit were found by me in the materials which had been brought up.

The thickness of the lower red sandstone, the Collyhurst sand, reaching to 423 feet, shows to what extent that deposit may be met with even on the outcrop of the permian strata. The boundary between it and the underlying coal-measures is difficult to determine, as no fossil organic remains were met with; but it was probably in the 5 inches of red marl, marked with an asterisk, as the coal-measures were then certainly reached, bright coal being found in some of the metals brought up by the boring-instrument.

In this section, the red marls, with beds of limestone and gypsum, and the Collyhurst soft sandstone, are both well developed; but no trace of the pebbly beds containing coal-plants, as met with at Astley and Bedford, was found. These last-named strata were also absent in the Chorlton-on-Medlock section, hereinafter described, as well as in the Seedley section, described at p. 103 of my second paper *.

Manchester Section.—Chorlton-on-Medlock.

In my first paper, before alluded to, as well as in another communication of mine printed in the first volume of the Transactions of the Geological Society of Manchester, this is described at considerable length. It commences near All Saints' Church in Oxford Road, and continues all

* Vol. xiv. (Second Series) of the Society's Memoirs.

the way past Medlock Vale to Waterhouses. Since the date of my former papers, at a point about a quarter of a mile to the south-west of All Saints' Church, marked A in the woodcut, a very interesting section has been met with in making a bore for water at the extensive sugar-works of Messrs. Fryer and Co. This bore, which is near 2 feet in diameter, was made by Messrs. Mather and Platt of the Salford Iron Works, with their improved apparatus, and as communicated to me by those gentlemen, is as follows:—

	ft.
Well in red sandstone (trias)	70
Bore in the same rock	40
Red and variegated marls, with thin bands of limestone	220
Coarse gravel and pebbles	43
Compact rock of red and white sandstone	20
Red and purple marls, containing bands of limestone	126
	519

The beds of limestone in the red marls afforded beautiful specimens of *Schizodus obscurus* and *Bakevellia antiqua*, *Pleurophorus costatus*, *Turbo*, and *Rissoa*; and the whole thickness of these permian marls was much greater than they had been previously met with near Manchester, except at Medlock Vale hereinbefore described,—their average having been generally about 130 instead of 220 feet, as in this section, and 245 feet at Medlock Vale. The lower red sandstone, on the other hand, was much thinner than when seen at Collyhurst or at Medlock Vale, hereinbefore described; and no pebble-beds containing fossil plants like those at Astley were found. But the most interesting features in the section are the red and purple marls, containing bands of what were called ironstones when first brought to me. On examination of these specimens, which turned out to be limestones, my attention was arrested by the occurrence of *Spirorbis* (*Microconchus*) *carbonarius*, and scales of a fish of the genus *Palæoniscus*, which, with their physical characters, clearly

proved them to be part of the upper or Manchester coal-field, in fact the Ardwick limestones—thus showing a band of carboniferous strata containing coal running under the trias and permian beds to the south of Manchester. This is of great importance, and proves that coal-measures are met with at places under trias and permian deposits much nearer the surface than was previously expected, and where the upper rocks give no evidence of their proximity. From the banks of the Medlock southward, the trias was proved to thicken, as shown at Messrs. Hoyle's works, Mayfield, where it was 143 feet 4 inches thick; and further south still, at the late Mr. Green's dye-works in Garratt Road, it was penetrated 324 feet in thickness, without going through it. The position of this bore is about 500 yards to the north of Messrs. Fryer and Co.'s. On taking the line of the great Irwell fault where last seen, in the workings of the Pendleton Colliery, and continuing it southward to the fault previously described in this paper at Heaton Norris, it would appear to run nearly through Messrs. Fryer and Co.'s works, and would account for the occurrence of the beds there met with; but the bore of Messrs. Worrall at Ordsal to the west, hereinafter alluded to, shows that the trias there is far thicker than we should expect would be the case, for it was penetrated 460 feet without its thickness being ascertained.

In continuing the great fault of Newtown and Collyhurst southward, and assuming that dislocation to have been made subsequent to the formation of the Pendleton fault, the permian and trias beds may have been thrown down, so as to account for a break in the great Irwell fault between Ordsal and Chester Street, and thus allow of the thickness of the trias at Ordsal. However it may have been brought about, there is no doubt that the Ardwick coal-measures last seen in the bed of the Medlock near Schofield's Chapel, and disappearing under the per-

mian and trias beds to the south, are brought up again by a fault running somewhere between Mr. Green's dye-works in Old Garratt and Messrs. Fryer's works in Chester Street, Oxford Road. This is one of the most interesting facts connected with the geology of Manchester that has come under my notice, and shows the desirability of continuing all the great lines of fracture in the coal-measures over the trias, to indicate the most likely places where that and the underlying permian deposits can be perforated, and coal-measures met with under them.

The remarkable features in this section are the great thicknesses of the red marls and limestones, and the conglomerate-beds, the one reaching 220 feet, and the other 43 feet. At Collyhurst and Newtown, these beds, described in my former paper, would not reach more than 120 feet and 1 foot 6 inches respectively. On the other hand, the soft red sandstone, which at Collyhurst was 320 feet, if represented at all in Chorlton-on-Medlock in the compact red and white sandstone, was only 20 feet in thickness. The lower permian beds of Astley, containing quartz, pebbles, and coal-plants, were not met with. This was also the case in the Seedley section*, to which this section bears the greatest resemblance of any in the immediate vicinity of Manchester.

Ordsal Section.

In my two papers previously alluded to, namely, those printed in Vol. XII. of the Society's Memoirs and in the first volume of the Transactions of the Manchester Geological Society, the various bores made in the trias around the city and the adjoining borough of Salford are, so far as they could be obtained, given. Amongst these is the well of our worthy President, Dr. Joule, F.R.S., near Albert Bridge. He was so kind as to oblige me with the section of the strata there met with. They were as follows :—

* Vol. xiv. (Second Series) of the Society's Memoirs, p. 103.

	ft.	
Trias, about	470	
Red clays, with limestones	120	Permian.
Red rock and clay, in alternate beds	10	
Lower new red sandstone	000	
	600	

In Water Street a bore was some years since made near the stables of the Manchester and Liverpool Railway, and a rock was reached similar to that met with at Dr. Joule's. This, from its description, appears to have been something like the conglomerate-bed seen at Cheetham Weir Hole, on the top of the lower sandstone.

At Messrs. Rothwell and Co.'s, in Water Street, the trias was penetrated 492 feet without going through it.

By the kindness of Messrs. Worrall of the Ordsal Dye Works, I am enabled to give the section of the trias beds which Messrs. Mather and Platt met with in their bore made at Ordsal some time since. It is as follows:—

	ft.	in.
Red rock	92	5
Red rock, mixed with white and a little raddle	7	0
Red rock	9	0
Red and white rock	5	9
Red sand and white rock	4	6
Red rock, very gritty	6	9
Red rock, not so gritty	6	2
Red and grey sandstone	9	7
Red and white sandstone	8	1
Red and white sandstone and raddle	7	2
Red and white sandstone	8	11
White sandstone	4	0
White sandstone, chiefly	17	0
Grey sandstone, very hard	3	10
Red and white rock	8	6
Red and white rock, hard and gritty, full of pebbles	6	8
Red and white rock, hard and gritty, very keen	5	8
Red and white rock, hard and gritty, more white	6	8
Red and white rock, hard and gritty, more white	12	2
Red and white rock, just through a raddle-bed a few inches thick	7	10
Carried forward	237	8

	ft.	in.
Brought forward	237	8
Red rock	12	10
Red rock, very hard	5	10
Dark red, very hard	14	0
Dark red	26	3
Raddle and red and white sandstone	7	2
Grey sandstone	13	3
Very red	5	3
Red sandstone and raddle	7	3
Red sandstone and raddle, and clay-band	8	6
Red sandstone and raddle, chiefly	6	0
Red sandstone and raddle, just entering raddle	4	6
Soft raddle and red sandstone, hard	14	6
Very hard, hardest found	18	9
Red sandstone	11	1
Stone and raddle mixed	7	11
Red and white sandstone	9	9
Much like a gravel-bed, as full of pebbles as possible	8	6
Red and grey sandstone	11	4
Red sandstone	29	6
	459	10

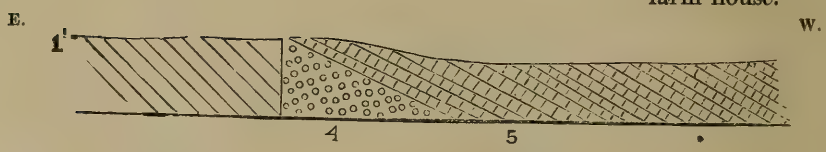
The bore was made for the purpose of procuring a supply of water for the works. Plenty of water was met with, but it was so salt that it could not be used for dyeing-purposes, and so the boring was discontinued. This is the only instance that has come to my knowledge of salt water having been found in the trias beds near Manchester. The result of the borings at Messrs. Joule, Rothwell, and Worrall's is given to show that in the line from New Bailey Bridge to Ordsal there is no evidence of the great Pendleton fault, although on continuing it south it ought to pass within that distance.

Since the date of my last paper, two new localities have been noticed where permian strata occur in West Lancashire. Mr. E. Hull, of the Geological Survey, first informed me of them. He has noticed them in the sheet which explains the geology of that district. My own ob-

servations were made before the publication of Mr. Hull's memoir.

Skillaw Clough Section.

(Distance about half a mile.)



This section is met with about a mile to the north of Newburgh Station, on the Wigan and Southport Railway. On entering the Clough from the upper part, we come to some flags and shales of a purple colour, which have a general dip of about 16° to the south-west. They continue several hundred yards, and appear to belong to the lower coal-measures. Just before they disappear, these strata dip due west at an angle of 25°, and are traversed by thin veins of carbonate of lime; then drift-clay intervenes, so that the strata are not visible for 10 yards, most probably owing to the occurrence of a fault in the interval. When the beds are again seen, we find a permian rock in the form of a dark-red sandstone, fine-grained and canky, which dips due west at an angle of 25°. Over 10 yards this sandstone is seen gradually becoming coarser in grain, and dipping to the west at an angle of 30°. It continues for 25 yards further, when its dip reaches 40° to the west, and no trace of a conglomerate-bed is observed. Then come 24 yards of red and variegated shaly clays, succeeded by a bed of yellow compact limestone 4 feet 6 inches in thickness. This is quite unlike any of the permian limestones of the south and west of Lancashire which have been previously seen by me; but it much resembles in chemical composition, colour, and structure the limestone

of Stank, described at p. 254 of my first paper*. The stone contains hollows filled with crystals of carbonate of lime, apparently the casts of shells. Although a considerable time was spent in searching for fossils, only one doubtful cast of a small shell resembling a *Schizodus* was met with.

The following is an analysis of the stone, for which I am indebted to the kindness of a friend:—

Carbonic acid	38·98
Lime	30·24
Magnesia	11·22
Silica	6·64
Iron, manganese, and traces of alumina	8·31
Water	1·10

Specific gravity, 2·75.

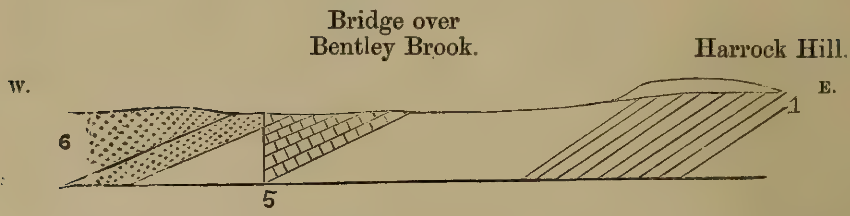
The dip of the stone was to the west, at an angle of 24°. For 220 yards the strata are not well exposed; but they appear to consist of red shaly clays; and in Mr. Thomas Eccles's orchard are seen some beds of red clays, containing thin bands of gritstone of a red colour, and lying nearly level. The country is flat, and the strata are covered up with drift-deposits; so that the beds of the trias, which most probably are there on the dip, do not show themselves, as exhibited in the woodcut.

* Memoirs of the Literary and Philosophical Society of Manchester, vol. xii. (Second Series), page 254. The following is a copy of the analysis, showing the composition of the Stank limestone:—

Carbonic acid	38·40
Lime	29·80
Magnesia	8·95
Silica	11·65
Oxide of iron	9·45
Water	1·75
	100·00

Bentley Brook Section.

(Distance about a mile.)



A little distance to the east of the last-named locality is another interesting permian section, not far from Bispham School. The rough rock is seen capping Harrock Hill, and dipping south at an angle of 25°; and on the inferior part of it the Haslingden or lower flags are found; but as we reach the flat ground below, for some distance east of the road over Bentley Brook, nothing but till is met with for about 500 yards. On the lower side of the bridge is seen a mass of dark-red shaly clays (permian), which, when they make their appearance lower down in the brook-course, dip to the W.N.W. at an angle of 45°. These strata, are about 20 yards in thickness. Then comes a bed of compact limestone, of a reddish-yellow colour, about 4 feet in thickness, exactly resembling that seen at Skillaw Clough, and lastly described. Next occur shaly marls of a red colour for about 40 yards, which gradually pass into a bed of red sandstone. These dip W.N.W. at an angle of 30°. Then a fault is seen running in a direction from N.N.W. to S.S.E. Near this fault the sandstone is hardened and discoloured; but for 100 yards along the brook-course a soft red sandstone, having pebbles of brown quartz, and evidently trias, is met with. This dips W.S.W. at an angle of 20°.

These two sections are of considerable interest, as they prove the existence of a magnesian limestone in the west of Lancashire where it had never been seen before. Most probably it is a continuation of the limestone seen at

Stank in Furness, as it resembles that deposit in all its characters, both physical and chemical. It differs much from the ribbon-beds of limestone found in the red marls of Newtown, Patricroft, Astley, Bedford, and Leigh, and bears more resemblance to the great central deposit of Yorkshire magnesian limestone. Now, can this bed lie above the former? It is possible that such may be the case, and it is desirable that every exertion should be used to ascertain the fact. The section at Stank, above alluded to, and the Rougham-Point section near Cartmel in Furness, described in my first paper, are the places where the question, Does the Stank, Skillaw-Clough, and Bentley-Brook limestone occupy the same position as the ribbon-beds of limestone near Manchester and Leigh? or is it a bed occupying a higher or lower position in the permian group? is likely to be answered by experimental bores.

Near Manchester, the permian beds occur in the following descending order:—

	ft.
1. Beds of red and variegated marls, containing thin beds of limestone and gypsum, affording *Schizodus*, *Bakevellia*, *Pleurophorus*, *Turbo*, *Rissoa*, &c., about	300
2. Conglomerate brown sandstone, about	50
3. Soft red and variegated sandstone (Collyhurst), about ...	500
4. Conglomerate sandstone (Astley), containing pebbles of white quartz and common coal-plants. This, with beds of finer-grained sandstone, all called lower permian, is about	60

The permian strata of the north-west of England are not so clearly classified as they ought to be. Some of the sections near Manchester, especially that seen in the Valley of the Irk, in Cheetham and Newtown, would apparently show that the red marls containing limestones and fossils of the genera *Bakevellia, Schizodus*, &c., passed into the overlying trias. This passage no doubt is only apparent, and not real; and, notwithstanding that the trias is seen reposing on the marls, it is most probable that

they are really unconformable to each other. The marls in the same locality appear to pass into the conglomerate, and the latter into the soft red sandstone of Collyhurst. These three divisions of the permian beds, although not always conformable to one another, are clear and distinct, and would hold good not only through Lancashire, but in Westmoreland and Cumberland. The marls are the only strata containing permian fossils, as the two latter strata up to this time have yielded none. Now, if the term permian were applied to these, no difficulty would arise; but when the coarse pebbly sandstones of Astley, lying under the last-named strata, and containing common coal-plants of the genera *Sigillaria, Lepidodendron, Calamites,* &c., beds very similar to those of the Ballast Quarry near Moira in the Ashby-de-la-Zouch coal-field, are included, we are obliged to distinguish them; accordingly, in my second paper, printed in the Society's Memoirs, they have been called lower permian. They occur just under the red marls containing limestones and fossils of the genera *Schizodus, Bakevellia,* &c., and where the conglomerate and soft red sandstone ought to have been, if those strata had been conformable to the overlying red marls. The lower permian are not only unconformable to the overlying upper permian, but also to the underlying upper or Manchester coal-field. The chief circumstance which induced me to remove them from the carboniferous strata was the conglomerate character of some of the sandstones, which are as full of white-quartz pebbles as millstone grits are. Now, in the Lancashire coal-field a considerable part of the lower, and the whole of the middle and upper coal-fields, comprising strata to the extent of 5000 feet, have never in Lancashire, so far as my knowledge extends, afforded a quartz pebble of the size of a pea; so the change of physical characters caused me to class the Astley beds under lower permian, rather than upper coal-measures. They undoubtedly occupy

the position of the lower Rothliegendes of Dr. Geinitz and the German geologists.

The Astley beds very much resemble similar pebbly gritstones seen in the upper part of the Pottery coal-fields of North Staffordshire, and have some resemblance to the Hooton-Roberts and Went-Bridge rocks of Yorkshire, as well as to those at Moira near Ashby-de-la-Zouch, before alluded to.

The Newtown and Bedford limestones to me appear, both by their physical and chemical characters, position, and organic remains, to resemble the thin-bedded limestones of Hooton Roberts, Hampole, Bolsover, and Kirkby Woodhouse, more than any other permian beds which have come under my notice. This opinion is different from that of my friend Professor King, of Galway, and other geologists, who regard these Lancashire limestones as the representatives of the higher magnesian limestones of Nottinghamshire, Derbyshire, Yorkshire, and Durham; but it is in accordance with the views of Mr. Kirkby, who has examined the organic remains of this part of the permian series with great care and ability.

IV.—*On Putrefaction in Blood.*

By Dr. R. Angus Smith, F.R.S., &c. &c.

Part I.

Read October 29th, 1861.

I have for a long time endeavoured to obtain some substantial results relating to the products of decomposition of substances in a putrid state. I have not, however, until lately proceeded in a direction such as to satisfy myself, and even now am only at the commencement of the subject.

I have already published some of my ideas on that condition of the atmosphere which may be called diseased. I have not believed that carbonic acid and sulphuretted hydrogen were capable of producing the results so frequently attributed to them, but agreed rather with the more ancient theory "of a conversive force in matter, to change the nature of things, by turning them into its own, when by immediate contact one body alters the properties, and changes the natural inward form and constitution and disposition of another, and works it to conformity to itself, draws it into its own likeness, impressing its own character upon it, and communicating to it its own form and nature" *.

In examining this subject, I passed air for several months through water, but did not obtain those decisive results which I sought. I then passed air through salts of lead, obtaining results which have been already published some years; in these, however, the organic matter was not estimated. When, however, the air was passed through a highly coloured and highly oxidized body, such as permanganate of potash, the influence of the organic matter became evident, and the relative condition of certain atmospheres was estimated.

By none of these methods, however, was the actual substance which has been so often sought really obtained. Its indications were viewed telegraphically; it was not handled or seen in a separate state. I therefore exposed blood to putrefaction, and caused air to bubble through it, obtaining in this way a more concentrated but a similar action. The air was then passed through a salt of lead as before. This time chloride of lead was used: it was desirable to use no organic acid, and no acid capable of oxidizing the organic substances. Carbonic acid came over in great abundance, and sulphuretted hydrogen. The sulphuretted

* From 'An Hypothetical Notion of the Plague,' by Mr. Place, 1721.

hydrogen was very abundant generally at first, but after a time it did not appear so; on allowing the blood to stand for a while, and then drawing air through it, the lead became deeply coloured by the sulphuretted hydrogen. There was, in fact, a solution of sulphuretted hydrogen formed in the blood; and as soon as the stream was allowed to pass through it, the sulphur compound was carried off. If, however, this were allowed to continue long, there was an excess of air, the sulphide of lead became oxidized, and a white powder was formed, which was sulphate of lead. It was strange how rapidly this oxidation took place. I had occasion previously to observe this rapid oxidation of sulphide of lead, in attempting to retain a permanent coating of that substance on the surface of lead pipes used for water. When the coating was made, I found it converted into a white powder in a day or two.

The air was passed through the putrid blood for several months, coming in contact immediately on leaving the blood with the chloride of lead. The salt of lead when examined was found to contain organic matter and ammonia; but it was not found to have lost all its putrid gases. It contained a very minute quantity of phosphoric acid. Carbon and nitrogen were found in the lead salt:

1·4 per cent. of carbon.

0·54 per cent. of nitrogen.

These amounts are as 100 to 38·5, instead of, as in albumen, 100 to 28·9.

We might at first infer from this that the ammonia is removed with greater rapidity than the carbon, and that there is not acid enough formed to retain it in the liquid; but this is not really the state of the case. The portion of the vapour retained by the salt of lead does not contain all the carbon which passed into it, whilst it contains nearly all the nitrogen. It might also be argued that, as only a

portion of the organic matter was retained, two separate substances of an organic kind existed in the vapour; but even this might not be fair, as water absorbs from the gas readily, and begins instantaneously to give out again. I draw, however, this conclusion, that there is a distinct amount of carbon other than that of the carbonic acid, and that a part of the bodies containing carbon is absorbed by acids, others by alkalies. Other experiments show that more is absorbed by alkalies. There was present, in fact, the substance of which I am in search, but of which I can give no account.

I was inclined to believe that when such a large amount of air was passed through the salt, not only was the sulphide of lead oxidized, as was plainly seen, but the organic matter was oxidized also, and perhaps a facility given to its oxidation by the presence of the salt. I believe this explains also why such a small amount of organic matter is obtained when impure air is passed through water. In the atmosphere itself the organic matter begins to be oxidized as rapidly as it is freed, and so prevents accumulation; and in passing air through water facilities for continued oxidation still exist undiminished, and perhaps increased. The method of freezing, so as to obtain the moisture and the dissolved matter at the same time, is very effectual, but it is a very troublesome operation, and, when it lasts long, it becomes expensive also. Besides, the actual vapours of putrefaction are not in this way obtained, on account of the rapid oxidation which occurs when they are mixed with the atmosphere.

It was in order to avoid the imperfect results alluded to that I adopted the simple method of enclosing the blood in a vessel, and collecting the gas which escaped under pressure. It might be said that this really does not express the exact condition of substances putrefying in nature; and I was deterred from it at first. But on consideration and on experiment it was seen that the blood required the as-

sistance of the air in order to continue putrefaction; and in nature it is generally found that putrefaction takes place in a very imperfect supply of air, in cases which in reality greatly resemble a closed vessel: the surface is generally exposed, but all below receives a very imperfect supply of air. In a similar way, the vessel was occasionally allowed air, whilst generally it was closed—the constant very slow supply in nature being equal to the occasional replenishing here adopted.

The amount of gas obtained by this method was not quite so large as I expected. It was needful to raise the temperature above that of the atmosphere during a large portion of the year. It seemed to me as if 54° Fahr. were a point to be marked especially. Below this there was little decomposition, above this a decidedly larger amount. However, the evolution of gas did not entirely cease when the temperature fell below 54°, neither did the increase arrive at its maximum on passing 54°. When the temperature rose towards 70°, the escape of gas was much more abundant; but this was rare, as the vessels were kept in an apartment not readily warmed, and they were too large to be moved frequently with safety. 55° Fahr. is generally marked temperate on our thermometers, and the characteristic of organic matter to be more active at this temperature is remarkable. The mere feeling of warmth and cold had long ago fixed on this point as marking the beginning of activity or inactivity in the materials of which we are composed. The feeling of cold arises no doubt from slowness in the decomposition of certain substances in the blood, and slowness of oxidation.

The relative amount of gas given off at different temperatures seemed to me a mode of measuring the relation of climates as far as danger from putrid substances is concerned, possibly also the production of disease. I have not obtained the amount of gas given off below 54° Fahr.; but

E 2

at 57° Fahr., or 16° Cent., the amount from a certain quantity of the blood was—

57°, lowest temperature	210 cub. cent.	1st day.
	100 „ „	2nd day.
	170 „ „	3rd day.
	100 „ „	4th day.
Rising up to 72°	5th day and 6th day	785, or 397 cub. cent. in a day.

The temperature of 57° was not constant; there was occasional rising; but that of 72° was maintained artificially. As 57° was the lowest observed here, I shall take 100 cub. centims. to be the amount at that temperature, and 397 the amount at 72°: a rise of 15° Fahr., or 8° Cent., is sufficient to increase the products of putrefaction fourfold.

When blood alone was used it became too thick, and the action of the atmosphere was impeded; it was a sealed bottle to itself; it was therefore mixed with about twice its volume of water and put into carboys so as nearly to fill them. The greatest care was taken to close the carboys, so as to allow none of the putrid gases to escape, as well on account of their unpleasantness and unwholesomeness as for the accuracy of the experiments. Nevertheless there was a constant unpleasant atmosphere in the apartment, which was the kitchen of the house, which I use as a laboratory, and which could be shut off from the other apartments. When vapours of this kind come in contact with solid bodies, a certain portion is left behind. In other words, we are not dealing with pure gases, but with gases and vapours readily condensable at the ordinary temperature, and having condensation greatly assisted mechanically by contact with surfaces. Water retains them, but smooth solid substances do so also. Furniture, walls, &c., exposed to such vapours, and porous substances such as clothes, retain them, and the long-continued action of the air is needed to ensure purification. For this reason walls require cleaning, and furniture must be rubbed; and a

room must not merely be exposed to a rush of pure air, so as to fill it, but it must be exposed to it long, in order to ensure complete oxidation. This would certainly lead us to suppose that in the air there was only a small portion of the oxygen which performed effectual duty—a sufficiently curious point, especially in relation to Schönbein's experiments. The first evolution of gas from the putrid blood is the most violent, as if the energy of life had scarcely left it; at least the force of that which held it together is diminished, and the change is more striking at the first moment of relaxing; or perhaps some of the same influences which held the particles of albumen together show their energy in breaking up the compound which a superior direction no longer compels them to retain unimpaired.

The first portions of the gas were measured with permanganate of potash only. The sulphuretted hydrogen destroys that salt with great readiness. The gas was collected over water, as the permanganate cannot be used with mercury. The water which was displaced from the tube into which the gas entered was treated also with permanganate. In each experiment 100 cub. centims. of gas were passed into an inverted tube containing water. The water destroyed the colour of a certain amount of permanganate, and the gas partly washed destroyed a certain amount in addition.

April 12	Liquid	14·5 cub. cent.	29·0
	Gas	14·5 " "	
April 12	Liquid	14·5 " "	29·5
	Gas	15· " "	
April 12	Liquid	13·2 " "	29·5
	Gas	16·3 " "	
April 17	Liquid	18·8 cub. cent.	39·1
	Gas	20·3 " "	
April 19	Liquid	18·1 " "	40·6
	Gas	22·5 " "	
April 26	Liquid	17·6 " "	41·0
	Gas	23·4 " "	
May 21	Liquid	35·3 " "	62·2
	Gas	26·9 " "	

About this time the amount of sulphuretted hydrogen in the gas was 6 per cent.

The absorbed gases were—

	Per cent.	Sulph. Hydr.	Residue.	
1.	82·68		17·32	Passed through metallic salts, SH removed.
2.	85·78		14·22	
3.	89·72		10·28	
4.	95·40		4·60	
5.	96·20		3·80	
6.	96·07	1·58	2·35	
7.	96·43	2·78*	0·79	
8.	97·62	0·06	2·31	
9.	97·09	1·93	1·98	

Note.—The figures obtained will be given at the end of the paper.

Another series was obtained with the following result, using as absorbing agents metallic salts and alkalies:—

	Gases absorbed.	Gases not absorbed.
1	97·08	2·92
2	97·27	2·73
3	97·69	2·31
4	97·71	2·29

The residue from 6 to 9 remains with little change. No. 7 appears anomalous, the amount of vapour of water not being known, and no provision having been made for calculating the gases free from it. Taking the amount of carbonic acid as 95 and that of sulphuretted hydrogen as 1·5, the amount of carbon will be 0·0513, and of sulphur 0·00218. The sulphur is to the carbon as 1 to 24·8, whilst in albumen it is as 1 to 33·4. The sulphur escapes more readily than the carbon, in proportion to its amount. The cause may be made quite clear when the whole amount in solution is ascertained; but the supposition of a part of the carbon undergoing a lower oxidation than in carbonic acid, will explain why less than an equivalent should escape when albumen decomposes.

It must be allowed that in all these there is a constant diminution of nitrogen, but no absolute proof of its elimina-

* +HO.

tion. It seems difficult to lower it below this amount, the putrefaction generally stopping still. A point to determine is, whether nitrogen is obtained from the albuminous compounds, or whether it be derived from the air needful for putrefaction.

It will readily be seen that it is not by mere oxidation caused by the oxygen of the air that carbonic acid is formed; the oxygen of the air is absorbed, but what becomes of it will be better known on examining the liquids. Certain it is that the carbonic acid comes off in overwhelming quantities, and some of it must be formed by the carbon and oxygen of the organic substances themselves coming forth and leaving the residue more carbonaceous than before. It is a transfer of much of the most solid elements of the blood into the atmosphere. But some oxygen is absorbed, and this oxygen takes up a certain quantity of carbon, which together form some of the carbonic acid which escapes. We cannot distinguish one part of a gas from another of the same kind; but the escape of carbonic acid on the disruption of the compound after oxygen has been absorbed, leads us rather to suppose that the act of oxidation had tended to liberate the gas. As less oxygen is absorbed than the amount escaping in carbonic acid, the whole mass of the blood must be losing oxygen along with the hydrogen and its compounds, and approaching a simple and inorganic form.

It was difficult to obtain sufficient for analysis of the unabsorbed residue, as there is only a small quantity, and that small quantity is chiefly nitrogen. When 21·3 millims. were obtained, it was found to consist of

Carbonic oxide	1·03 or	4·8	per cent.
Carburetted hydrogen	0·54 or	2·5	,,
Hydrogen	1·3 or	6·2	,,
Nitrogen	18·43 or	86·5	,,
		100·0	

The amount of carbonic oxide and carburetted hydrogen

gases is extremely small. It may be interesting to inquire whether they are products of the decomposition of bodies existing only in very small quantities in the blood, or whether, during the stage of decomposition, a force is exerted sufficiently powerful to break down small portions of well-known organic products into gases such as the above, but insufficient for more.

Gases after passing through solutions of Metallic Salts.

	Vol.	Pressure in mm.	Temp. (Cent.) °	Vol. at 0° and 1000 bar.
No. 1	63·4	550·7	17·3	32·8
After absorption of carbonic acid	12·5	482·0	17·7	5·65
No. 2	72·4	532·0	17·7	36·17
After absorption of CO_2	8·6	481·7	17·2	3·72
No. 3	100·9	567·6	16·9	53·92
After absorption of HS	103·1	560·8	16·9	54·44
After absorption of CO_2	16·9	488·2	16·9	7·67
No. 4	181·1	651·8	18·05	115·01
After absorption of CO_2	11·6	496·3	18·05	5·33
No. 5	134·7	603·1	16·9	76·51
After absorption of CO_2	6·4	485·1	18·3	2·91

Gases from Blood.

	Vol.	Pressure.	Temp. °	Vol. at 0° and 1000 bar.
No. 6	198·7	707·2	16·9	132·4
After absorption of HS	197·4	702·4	17·5	130·3
CO_2 removed	6·4	522·8	17·8	3·142
No. 7	210·6	725·3	18·0	140·2
HS removed	209·4	717·8	18·9	136·3
CO_2 removed	8·7	522·6	18·3	1·1

The Gas left in the tube.

	Vol.	Pressure.	Temp. °	Vol. at 0° and 1000 bar.
No. 8	221·5	732·6	18·3	147·84
HS removed	220·9	734·9	17·8	147·74
CO_2 removed	15·3	533·0	16·6	3·43
No. 9	177·1	673·5	13·3	113·7
HS removed	174·3	670·7	13·0	111·6
CO_2 removed	13·6	752·1	12·8	1·2

Residual Gas after being deprived of CO_2, HS, NH_3, and any substance absorbed by Potash.

	Vol.	Temp. °	Vol. at 0° and 1000 mm.
	56·3	15·5	21·31
+O	95·4	15·5	38·96
+H	118·6	15·5	51·09
After explosion	75·3	15·8	29·33
CO_2 removed	69·8	14·7	27·76
+H	181·4	15·0	91·96
After explosion	138·4	15·0	63·98

Part II.

Read April 15th, 1862.

When I began to examine the products of the putrefaction of blood, it was with the object, first, of ascertaining the nature of the gases; and next, of ascertaining whether any matter in them exists in a so-called organic condition, and, if so, in what quantity. I have ascertained the nature of the gases; so far as I see, however, I have added no new one; but I believe that for the first time I have given the proportionate amount of each. After the decomposition had proceeded to such an extent that it was difficult to obtain even a few bubbles more, the gases existed in the following proportion:—

Carbonic acid	97·09
Sulphuretted hydrogen	1·93
Hydrogen	0·1804
Carbonic oxide	0·1396
Carburetted hydrogen, CH_2	0·0729
Nitrogen	2·5171
	100·

Progress of the decomposition.

	Absorbed by lead and potash.	Not absorbed.
Nov. 9.	88·65	11·35
12.	91·32	8·68
13.	91·56	8·44
14.	95·90	4·10
15.	96·04	3·96
18.	98·26	1·74
19.	98·50	1·50
20.	98·95	1·05

Other experiments gave me much more hydrogen; and I am prepared for a considerable variation in the amount of the several gases. The nitrogen came to a minimum whenever the decomposition became slow. This might be interpreted in two ways: first, by the absence of air to continue the process; and secondly, by the absence of nitrogen from the decomposed albumenoids. I do not see from my experiments a sufficient proof of the elimination of nitrogen, as the process stopped on all occasions at the time when I should have supposed the atmospheric air to have been removed, except perhaps in the series

CO_2 and absorbed gases	117·47	124·02	144·49	148·14
Residual gas	3·53	3·48	3·41	3·46
	121·00	127·50	147·90	151·60
= {	97·08	97·27	97·69	97·71
	2·92	2·73	2·31	2·29
	100·00	100·00	100·00	100·00

which seems to show a constant amount of nitrogen freed by decomposition. The production of pure hydrogen and of compounds of carbon and hydrogen is interesting, and leads us to consider the part which they play in nature, where they are no doubt used in such a manner as to lead them to recombination. The nitrogen is probably both eliminated and absorbed in the action of organized bodies.

This short paper is only a slight addition to the former one, establishing, to my own satisfaction at least, the fact of the existence of organic compounds along with the gases. I have not, however, prepared them in sufficient quantities, although I have been able to find a method of doing so without great difficulty. The reason I have not completed a portion of the inquiry so apparently within reach, may be called a personal one, but it is not without interest. The gases were extremely offensive; they pervaded the laboratory for months, and every corner was affected; the moment the door was opened strangers were offended;

and even those who were constantly present were not quite insensible to the evil. In operating with them the odour was intolerable to me, and I was frequently obliged to leave at an important moment for a breath of fresh air, whilst the nausea was prolonged nearly to vomiting. My assistant did not feel much annoyed, or I would not have asked him to continue the work. On his leaving me, I was not inclined to continue, at least without a considerable interval of pleasanter work; and this I found in a branch of the subject relating to the absorption of gases.

I mentioned that by passing the gas through lead and other metallic salts, only a small amount of organic matter was collected; but by passing it through caustic potash the amount was considerable. A flocculent matter fell, but the chief amount remained in solution. The solution was boiled down; and when warmed, a perfectly fresh odour of soup was spread through the room; everything offensive had been removed, and the smell was for the first time very agreeable. Here we find that the substances sought for are decomposed by the very means which we take to retain them. But in this experiment we see a demonstration that substances of an organic nature pass over with the gases. When strong sulphuric acid was added to the potash solution, there was an abundant black precipitate of carbon, showing that more than enough for analysis had been prepared. There was a fatty odour from it when sulphuric acid was added, leading me to think of some of Chevreul's remarks on a similar occasion.

As these compounds were not retained by acid salts but by alkalies, I concluded that they were acid. But on allowing some of the solution to stand for a few hours, I was surprised to find that the organic matter had almost disappeared. This is the action of a neutral body.

We see clearly how differently this substance acts from carbonic acid. I took home a small piece of cotton-wool over which the gas had passed for some days; my intention was to examine it with the microscope. Less than a grain of this cotton was taken out of the tube in which it was enclosed; but so thoroughly did the room become offensive, that some friends, not aware of my pursuits, were much annoyed.

This leads to the conclusion that the amount of carbonic acid is entirely incapable of showing the true condition of an atmosphere, unless we estimate that gas at once on its formation, as then it is mixed with organic matter, if it is formed out of animal substances; if, however, we allow even a short time to pass, a separation takes place, the carbonic acid diffuses, and the organic matter clings to surrounding substances. If the gas were previously passed through charcoal, it was difficult to obtain a trace of organic matter.

A trace of a compound of cyanogen was found, and a small amount of phosphoric acid was obtained in the acid solution. Ammonia was found in considerable quantities. These substances exist along with the gases, and are all I have hitherto determined.

If carbonic acid were a measure of atmospheric impurity of any real value, then the result would be that there are no unwholesome atmospheres in nature; but it is one so gross and valueless, that it is only found in excess when it is already known by loss of strength, or of life itself, that the air is pernicious.

In estimating the carbonic acid and the sulphuretted hydrogen, we include the organic substances. A portion of the latter, as has been seen, is taken up by the acid salts of metals; and when the residue is passed over soda, the smell is entirely removed and gases remain. These organic substances seem to be the truly injurious portions of the

putrid matter; and to throw light on them is the object of further inquiries. Metallic salts by no means remove them when formed, although they may prevent their formation by obstructing decomposition. But metallic salts remove the sulphuretted hydrogen, it is therefore shown that this gas is no essential element in a putrid odour; it is even probable that it may tend to diminish its virulence.

The question as to the physical conditions of these substances presses for an answer. I have pictured them to myself in some cases as particles hollow or otherwise. When we make hydrogen from water by zinc and sulphuric acid, a number of particles rise from the bubbling liquid; some fall back, others float onward. At a considerable distance we smell hydrogen; but it is not only the gas we smell, we become sensible of a considerable irritation, and of sulphuric acid floating in the air. When a bubble rises, it seems to take along with it a little of the acid in the form of a soap-bubble, and floats like one. It is extremely probable that when it has floated long it loses the vesicular state, and becomes concrete, as De Saussure terms such a drop; at least we might expect this from the constitution of such a globule. I should suppose the same thing to occur in all rapid evolutions of gas, this necessarily depending much on the tenacity of the liquid. If minute vesicles rose, they would become liquid globes after a time; and if these, by evaporation, lost their liquid, they would leave in the atmosphere solid shapeless particles, perhaps such as the microscope reveals. This may be one method by which substances become coated over with putrescible matter. That the substances are carried into the air we have other proofs, not even forgetting the despised one, that some insects live in the vapour, and seem to obtain no other food. Wherever many flies are found, there is to be found a large amount of matter affecting the sense of smell; and great cleanliness causes them to leave a house.

But there are other cases when evaporation occurs and a liquid or solid is said to assume the gaseous state: we imagine it to assume that state by the atoms at the surface separating to great distances, not by expanding into a gas below the surface, as in boiling or in effervescing, and rising up with a mechanical force ready to take a globe of liquid along with them. At the same time the question naturally arises, at what period of boiling or effervescing does this vesicle form or cease to form, assuming that it does so? If watery solutions of many substances be boiled, solid matter is taken up readily. Is there a time when evaporation is so calm that no vesicles can be formed, and no solid matter raised? This we cannot doubt. Again, is the solid matter taken up by an agency more allied to chemical than mechanical laws? In examining these two classes of laws, we generally find that they run into each other on their frontiers in such a manner as to make a definite line impossible; and I think it would be unwise to suppose that in this case there would be an exception. I would then suppose a series of methods. The first is the formation of vesicles of visible size, as described: we have in them light bodies ready to decompose, making their escape when the surface of water rises; for with water they have been hitherto united, and they may somewhat retain their connexion even when that water has somewhat enlarged its boundaries. It is exceedingly probable that the evaporation of a large quantity of water raises in this way a small portion of the bodies with which it was united. Such substances cannot long maintain their independent position in the atmosphere; they must be removed from it. The last stage would be, that every minute particle should assume the gaseous condition when it had sufficiently separated itself from other bodies, or had become sufficiently liberated from their attraction—at the surface, for instance, of the liquid.

Even while writing this, I am aware of many opposing facts and opinions. Whilst the surface-water of the ocean contains a considerable amount of organic matter, I have not found more in the atmosphere, on a calm day, over the sea than over mountains, but a large amount during a slight wind. In working on putrid matters, the vapours pass through porcelain which is unglazed, though very compact, under circumstances which seem to forbid anything but the most perfectly gaseous matter to rise. I am aware, too, that I must not be led away by analogies to suppose that bodies in the state of vapour cannot undergo putrefaction; and I will even go further, and say that the affirmative hypothesis would explain a larger number of phenomena as they appear to us at present. We must beware of fancies in this dark region.

P.S. I may mention that even 130° Fahr., nearly 55° Cent., does not prevent putrefaction, which probably ceases at 140° Fahr., 60° Cent., the point of the coagulation of albumen.

V. *On certain Scales of some Diurnal Lepidoptera.*

By JOHN WATSON, Esq.

Read before the Microscopical Section, November 18th, 1861.

THE scales of lepidopterous insects have long been subjects of microscopical examination; but it may be questioned whether sufficient notice has hitherto been taken of their peculiarities, with a view to the determination of the genera, species, and affinities of the insects, or of their systematic functions.

The ordinary scales are more or less oval, showing from 2 to 5 or more dentations at the broader end, and having a short, stiff, pointed peduncle at the other extremity, by

which they are attached to the membrane of the wings. These scales are flat, like those of fish, and show striated markings. Referring to them in his 'Introduction to the Classification of Insects,' Westwood says, "Lyonnet has filled several quarto plates with representations of these scales, varying to almost every form, taken from the wings and body of the Goat Moth; so that the suggestion of a writer, that the forms of these scales might be used for specific characters, is entitled to no weight." (He likewise refers to a paper upon the same subject by a French author, presently to be noticed.) But at the time this was written the microscope showed all the scales as nearly flat; and now the binocular instrument enables observers to discover rotundity where it was not previously suspected. By help of the above instrument it appears probable that two or more different kinds of scales, serving distinct and separate offices, are to be found in lepidopterous insects; and this difference of function has not hitherto been suggested.

In some genera of the diurnal Lepidoptera, besides the ordinary scales, some peculiar forms exist; and it is to these attention is now to be drawn, especially to those found in the genus *Pieris* and its congeners. Examination with the binocular microscope shows that these scales are not flat like the others, but cylindrical and hollow; they are attached to the wings by a bulb, at the end of a thin elastic peduncle differing in length in different species. The bulb also varies in size and shape; and there is a hole or indentation to receive it in the membrane of the wing, larger than that for the ordinary scale; and the whole apparatus has the appearance of a ball-and-socket joint, allowing considerable facility for motion or play. The scales are fixed to the wings at the broader instead of the narrower extremity, and there they are furnished with a fringe of cilia or hairs. The scales are placed on the upper surface of the wings, principally on the superior ones, with their

tips projecting between the common scales; they are easily detached, and in removing them the bulb is very liable to be broken off; they are much more numerous on some species than on others, and their number varies considerably even on individuals of the same species, especially at different periods of existence. The males alone possess them; none are ever found upon the females. They have been called "plumules" by some authors; and those of *Pieris Brassicæ, P. Rapæ,* and *P. Napi* (our common white garden Butterflies) are well known to microscopists, and were formerly called test-objects.

In the 'Annales des Sciences' for February 1835, there is an interesting article on the organization of the scales of Lepidoptera, by M. Bernard Deschamps. It is principally devoted to the consideration of the structure of the scales, as composed of several lamellæ or membranes, of the mode in which they are affixed to the wings, and of the place in which the colouring matter is deposited. He also refers to these plumules, and gives figures of a few of them: he does not suggest any peculiar use for them, but draws attention to the fact that the males alone possess them, and that they have some general resemblance, with certain specific differences. He examined and figured seven species of the Pieridæ, to which family I am about to allude. My friend Mr. Sidebotham has most kindly and laboriously drawn the plumules of about one hundred species of Pieridæ observed by me, very few if any of which have been figured before.

The most remarkable of these forms are represented on Plates II. & III.; and a list of the names of the genera and species to which these forms of scales belong is appended to the paper.

To the same gentleman I am also indebted for many valuable suggestions carried out in the preparation of this paper. It must be understood that the drawings are not made to scale with any one power of object-glass; the

relative sizes of the plumules are various, always, however, about the same in different individuals of any one species. The smallest or shortest are about 5-100ths of an inch in length, and the largest or longest about 1-100th, including the pedicle and bulbs.

According to the modern arrangement of Doubleday, Westwood, and Hewitson, the family "Pieridæ" consists of 16 genera; and in 7 of them, viz. *Euterpe, Pieris, Anthocaris, Idmais, Thestias, Hebomoia,* and *Eronia,* I have discovered plumules*. There are several distinct types of plumules, generally more or less running into one another; but each species possesses its own peculiarity, with diversity sufficient for identification, while in each individual of the same species there is always the same form of plumule. These, therefore, must afford to the scientific entomologist a valuable test in the determination of closely allied species, and it is probable that they may serve to form congenial natural groups and subdivisions in some of the genera.

It is remarkable that the peculiar and well-known plumule of *Pieris Rapæ* should prevail, in a generic and very similar form only, also in *P. Napi, P. Cruciferarum,* and *P. Gliciria* (the first two European, the third North American, and the fourth Chinese). These insects are of close affinity in other respects; and in other instances congeniality of plumule is found in nearly allied insects.

The figures 20 and 21 deserve particular notice, as exhibiting a form which appears to be peculiar to the genus *Euterpe.* This form may perhaps be considered a test for that genus, and I believe that it has not been noticed or figured before.

The most remarkable and beautiful form of plumule, now for the first time observed, as far as is known to the writer or others to whom it has been shown, is that found

* The examination extended over about 200 species; and I found plumules in all the male specimens, with three exceptions.

on *Pieris Agathina* and *P. Chloris,* two West-African Butterflies; and no approach to this form has been discovered on any others. The figure drawn by Mr. Sidebotham (fig. 15) gives a very correct idea of the reality; but the study of the actual objects with the binocular microscope and high powers will be well rewarded, and give abundant cause for speculation as to the absolute form of the plumules. They appear to be hollow membranous bags of a cylindrical or triangular shape, bound round by longitudinal ribs, which are curved inwardly, forming a contraction at about one-half or one-third of their length, where they are drawn in as by a cord. At the base, the ribs are inflexed towards the peduncle and bulb, to which they seem attached by the membrane. The large double-lobed transparent bulb, besides acting as a ball-and-socket joint, seems to serve as a valve to close the bag. Above the contraction, the ribs are continued with a curvature similar to the lower portion, and terminate in extremely fine and delicate points. In different specimens these approach more or less closely, and they appear to be free at the upper extremity, with a power of contraction or closing to protect the interior of the bag from the entrance of injurious matter. Their appearance is very much like that of the ciliated tentacula of the *Stephanoceros* or peristomes of some of the Mosses. The length of the bag is about 1-300th of an inch, without the peduncle and bulb, which, when fully drawn out, extend about 1-800th of an inch further beyond the point of attachment. Passing from the Pieridæ to the very extensive family Nymphalidæ, it must be noticed that the plumules have only been discovered on one genus, viz. *Argynnis.* The type or character of form is very distinct from any in the Pieridæ; but all the species examined exhibit generic resemblance and specific variety; and it is probable that the plumules may be discovered in other genera.

In the family Satyridæ, the plumules have been found in several genera, as shown by M. Deschamps; the type is again distinct, and is well known to microscopists as a scale of our common meadow brown Butterfly (*Hipparchia Janiva*), and possesses similar relative varieties.

The battledore scales of *Polyommatus,* long known to microscopists, are of a form differing from any others, again with generic similarity and specific variation. They may perhaps serve the same office as the plumules, but whether or not they have the same mode of attachment requires further investigation.

Next comes the interesting question concerning the function of these plumules in the economy of the insects, and the purpose they serve beyond that of the ordinary scales, which seem to act as the feathers of birds, in guarding the insects from wet, and supporting them in their flight—unless, indeed, they are not more nearly allied to the scales of fish. Reaumur and some other entomologists have supposed that the common scales, in addition to these ends, supply the tracheæ in the nervures of the wings with air, and that the striæ show the channels or air-passages; but after close examination of them with high powers, no external openings have been found fitting them for this purpose. The plumules, on the contrary, appear admirably adapted for air-vessels: they are hollow, and can be inflated like balloons, and have a tuft of cilia at the summit, which, by constant oscillation, may prevent hurtful substances from entrance, just as the cilia in the spiracles of many insects act. Through the bulb, which is valve-like-shaped, being divided into two lobes, there may be communication with the tracheæ. The plumules may thus perform a double function, conducting a supply of air to the nervures of the wings, and, when inflated, adding considerably to the buoyancy of the insect. Besides, from the manner in which they are placed,

partly between and partly under the ordinary scales, the latter must be raised when the former are inflated; and when not in use, they probably lie flat, like empty bags, under the superincumbent scales. By this supposition, as regards the functions of these plumules, we may account for the superior strength and power of flight which the males possess over the females.

Here, then, is a field open for great microscopical research—a field which promises variety of interest the further it is pursued. New forms of scales will probably be discovered in many genera hitherto unexamined; the attention should not, however, be directed solely to the observation of these plumules, as all the forms of scales are worthy of careful study.

Annexed is a list of the plumules figured on the Plates by Mr. Sidebotham, together with the geographical habitat of the insect.

PIERIS.

	Name.	Locality.
1.	Hirlanda.	Bengal.
2.	Pyrrha.	Brazil.
3.	Zochalia.	South Africa.
4.	Teutonia.	Australia.
5.	Phryne.	Java.
6.	Hedyle.	West Africa.
7.	Argenthona.	Australia.
8.	Gliciria.	China.
9.	Belladonna.	North India.
10.	Harpalyce.	Australia.
11.	Belisama.	Java.
12.	Lanassa.	Australia.
13.	Isse.	Celebes.
14.	Temena.	Lombock.
15.	Agathina.	West Africa.
16.	Gidica.	Senegal.

ANTHOCARIS.

	Name.	Locality.
17.	Ione.	Senegal.

THESTIAS.

18.	Mariamne.	India.
19.	Venilia.	Java.

EUTERPE.

20.	Swainsonii.	Brazil.
21.	Charops.	Mexico.

ERONIA.

22.	Argia.	West Africa.
23.	Valeria.	North India.

HEBOMOIA.

24.	Glaucippe.	China; India.

List of all the Species drawn by Mr. Sidebotham.

PIERIS.

	Spec. name.	Habitat.
1.	Hirlanda.	Bengal.
2.	Sylvia.	West Africa.
3.	Paulina.	Java.
4.	Lorena.	Quito.
5.	Coronea.	Java.
6.	Lypera.	Venezuela.
7.	Pyrrha.	Brazil.
8.	Monuste.	West Indies.
9.	Gidica.	Senegal.
10.	Charina.	South Africa.
11.	New species.	Celebes.

Pieris.

Spec. name. Habitat.

12. New species. Celebes.
13. Melanita. Australia.
14. Eleone.
15. Teutonia. Australia.
16. Creona. Bengal.
17. Nabis. Australia.
18. Cratægi. Europe.
19. Calydonia. Venezuela.
20. Habra. Honduras.
21. Phryne. Java.
22. Zarinda. Java.
23. Nero. Java.
24. Margarita. Brazil.
25. Ithome. Celebes.
26. Calypso. West Africa.
27. Eudoxia. West Africa.
28. Hedyle. West Africa.
29. Antonoë. China & N. India.
30. Eucharis. China & N. India.
31. Argenthona. Australia.
32. Vishnu. Java.
33. Napi. Europe.
34. Cruciferarum. U. States.
35. Rapæ. Europe.
36. Gliciria. China.
37. Nigrina. Australia.
38. Belladonna. North India.
39. Egialea. Java.
40. Pasithöe. China.
41. Agostina. East India.
42. Thisbe. China & N. India.
43. Aganippe. Australia.
44. Harpalyce. Australia.
45. Descombesii. North India.
46. Belisama. Java.
47. Coronis. China.
48. Lea. Borneo.
49. Lanassa. Australia.
50. Buniæ. Brazil.
51. Amasene. Java.
52. Brassica. Europe.
53. Agathina. West Africa.
54. Chloris. West Africa.
55. Zochalia. South Africa.

(56 to 62, see opposite.)

63. Ada. New Guinea.
64. Calydonia. Venezuela.
65. Climbra.
66. Cycinna. New Guinea.

Spec. name. Habitat.

67. Demophile. Brazil.
68. Liberia. Amboyna.
69. Lypera. Venezuela.
70. Melania. Australia.
71. Mesentina. Australia.
72. Nerissa. Java.
73. Pylotis. Brazil.
74. Soracta. North India.

(75 to 87, see below.)

88. Aripa. Caraccas.
89. Elodia. Mexico.
90. Hellica. South Africa.
91. Hyparete. Java.
92. Philyra. New Guinea.
93. Vishnu. Java.
94. Dorimene. Amboyna.
95. Isse. Celebes.
96. Nysa. Australia.
97. Temena. Lombock.
98. Aspasia. Manilla.

Anthocaris.

56. Eucharis. India.
57. Ione. Senegal.
58. Genutia. United States.
59. Belia. Europe; N. Africa.
60. Cardamines. Europe.
61. Danaë. Bengal.

Thestias.

62. Mariamne. India.
75. Pyrene. China; India.
76. Venilia. Java.
77. New species.
78. New species.
79. New species.

Hebomoia.

80. Glaucippe. China; India.

Euterpe.

81. Swainsonii. Brazil.
82. Leucodrosine.
83. Charops. Mexico.

Eronia.

84. Leda. Port Natal.
85. Cleodora. South Africa.
86. Valeria. North India.
87. Argia. West Africa.

VI.—*On the Tongues of Mollusca.*

By Thomas Alcock, M.D.

Read March 18th, 1862.

In introducing to your notice the tongues or lingual ribbons of the Mollusca, my chief purpose is to give some slight idea of their immense variety, and their great beauty as microscopic objects; and with this view I have selected specimens of between twenty and thirty different species for your inspection; but, of course, when you take into account that every distinct kind of Mollusk which possesses this lingual apparatus has, so far as we know, a different pattern of teeth, you will see that a small series like the present can give only a very faint notion of the almost endless variety of beautiful objects which may be obtained by following out these inquiries. If, however, these serve to recommend the subject practically to your notice, they will quite answer my intention in showing them.

You are probably aware that the scientific use to which the examination of these curious organs has been applied is to assist the conchologist in the classification of shells, especially by serving as a test to distinguish in doubtful cases between true affinity and mere similarity of general form, which is a constant source of difficulty when only the shells of these creatures are examined.

Since the publication of Dr. Lovén's work on the dentition of the Mollusca, which strongly directed the attention of conchologists to the subject, several improved systems of classification have appeared, in all of which the lingual dentition forms an important element; but I believe it is generally felt by those who have studied these works, that although they show an immense amount of labour, they leave the subject in a very unsatisfactory state,—all the facts that have been collected, however,

tending to show that a steady prosecution of still further researches is likely to lead to most valuable results.

This being the case, I have devoted my spare time for more than a year past to the dissection of Mollusca; and one or two of the results, as regards lingual dentition, I now propose to lay before you.

The lingual ribbon is found in all the Gasteropoda, that is, in all those Mollusca which, like the Snail, crawl on a broad, flat disc, forming the lower surface of the body; and the special characters which are found to belong to the organ in different kinds of Gasteropods have been applied to classification for the three following purposes:—to distinguish between very nearly allied species, to determine the true limits of genera, and to form these genera into natural groups or families.

The *orders* into which the class Gasteropoda is divided were well established by Cuvier on the characters of the breathing-organs, and, according to his arrangement, they were eight in number; but these have since been thrown together by Milne-Edwards into three groups, founded upon certain broader features of the same organs, which are still admitted by all conchologists to furnish satisfactory distinctions for the division of the class into orders.

The series of specimens to which I wish to call your attention this evening belong to two of the three orders of Milne-Edwards, namely, the Prosobranchiata, or those which have the gills in front of the heart, and the Pulmonifera, or those which breathe air; and the first thing I have to remark about them is, that you will find, on examining the series, that, while they all differ considerably from one another, they form themselves into *four* very distinct and natural groups, with characters so well marked that there cannot be a moment's hesitation in deciding to which of the groups any specimen belongs.

The first are distinguished by having an immense number of very minute teeth, arranged in long transverse rows, and all similar in shape, except the central ones. These belong to animals contained in the order Pulmonifera of Milne-Edwards, which corresponds with the Pulmonata of Cuvier; and the illustrations I have prepared are tongues of *Limnæus stagnalis, Helix pomatia,* a species of *Bulimus* from California, and *Siphonaria gigas,* a very interesting sea-Mollusk, with a shell like a limpet.

The three other groups, which are quite as distinct from one another as they are from that just mentioned, belong to animals all of which are thrown together into the one order Prosobranchiata by Milne-Edwards; but in Cuvier's arrangement we find a separate order corresponding with each.

The first of these I shall mention is his order Cyclobranchiata, including the Limpets and several allied genera, which all possess a peculiar type of tongue, distinguished by its great length and by several well-marked characters in its teeth. In the first place, there is no tooth in the middle line, but they are arranged symmetrically on each side of it, in alternate central and lateral sets, so that, strictly speaking, two rows go to form one complete series (Pl. IV.); then the teeth themselves are remarkable for their dark-brown colour, their strength, and the bold manner in which they project from the membrane, reminding one of the strong prickles on a rose-bush. The specimens illustrating this type of teeth are, *Patella vulgata,* one of the preparations showing the entire length of the ribbon, and accompanied by the shell of the animal from which it was taken; *Patella pellucida,* which, compared with *Patella vulgata,* affords a good example of strongly marked *specific* differences; and several American species, including *Patella, Scurria,* and *Acmæa.* The Chitons, which were placed in this order by Cuvier, have been properly separated by later naturalists.

Next comes the order Scutibranchiata, including the Trochuses, Earshells, Fissurellæ, and some others. The ribbon in these is comparatively short and broad; and the teeth in each tranverse series are generally very numerous, and of three distinct kinds: in the first place, a median set, consisting of a central tooth, with four or five similar ones on each side of it, all having a lamellar form, with their body laid flat on the supporting membrane, the recurved points only projecting; secondly, one or more large, strong, dentated teeth on each side, standing up boldly and overarching the median set; and thirdly, a numerous series of long, slender, brush-like teeth, flanking the overarching ones on each side, and generally forming a full fringe down the borders of the ribbon. These particulars may be seen in Pl. V. The specimens by which I shall illustrate this type of teeth are tongues of three species of Trochus, which, by comparison, will give good illustrations of specific differences, the tongue of an Earshell from California, of *Fissurella nigropunctata* from Nicaragua, and of *Glyphis inæqualis,* an animal which, from the characters of the teeth, must evidently be quite distinct from *Fissurella,* although the shell is similar.

The third and last order I have to speak of is that of the Pectinibranchiata, which includes a great majority of all the marine spiral univalves, these being linked together by a common type of general structure; but still, as might be expected in so large a number of forms, they present strongly marked differences in detail, by which the order is subdivided into minor groups. Evidences of the existence of these distinct groups are at once seen on looking over even a small series of their lingual ribbons; and in Pl. VI. a few of these varieties in the character of the teeth are represented. One very marked difference, as you will at once notice, is that, in some sets, the teeth are in transverse series of three, while in others there are seven

in a row; but at present my object is to define the characters they have in common, and to show that they are perfectly distinct from those in the orders previously mentioned. The general character of this form of ribbon, then, is that it consists of three longitudinal bands of membrane laid side by side—a central and two lateral ones, the central band being armed with one tooth, and each lateral with either one or three, in the transverse series. In a few families, I may mention, some or even all these teeth are wanting. The specimens by which I shall illustrate this order are tongues of *Fusus antiquus*, *Buccinum undatum*, *Nassa reticulata*, *Purpura patula*, *Cerastoma Nuttalli*, *Natica monilifera*, *Littorina littorea*, *Cypræa albuginoides*, and *Luponia vitellus*.

Some of those who have given their attention to the lingual dentition of the Mollusca have expressed doubt as to its trustworthiness as a guide to the essential organization of the animals. Woodward, for instance, says, "The patterns or types of lingual dentition are on the whole remarkably constant, but their systematic value is not uniform. It must be remembered that the teeth are essentially epithelian cells, and, like other superficial organs, liable to be modified in accordance with the wants and habits of the creatures. The instruments with which animals obtain their food are, of all others, most subject to these adaptive modifications, and can never form the *basis* of a philosophical system." Other authorities hold the opposite opinion quite as strongly; but, after all, the question is not one to be decided by an opinion, but by facts; for what the conchologist wants to know is whether, as the result of observation, the lingual ribbons can be relied on or not, and if not absolutely, still to what extent. This being the state of the case, it is necessary to move step by step in the inquiry; and the first point to decide is, whether there is a characteristic form of ribbon

belonging to each of the main divisions or orders. This I have shown, so far as my present materials will admit, is really the case; and my illustrations at the same time point out that Cuvier's order Cyclobranchiata must be reestablished as distinct, and not included, as it is by our latest authorities, Gray and Adams, in the order Scutibranchiata, to which, on the evidence of the teeth, it cannot possibly be related; and I may remark that, judging from the specimens of each order which I have had the opportunity of examining, they are equally distinct in their general anatomy.

I do not intend, on the present occasion, to go much into the details of lingual dentition; but there are one or two points regarding some of the animals of the order Pectinibranchiata which I will take this opportunity of mentioning. *Fusus antiquus* and *Buccinum undatum* are animals of different genera, but both are known under the common name of Whelks. Gray says, "The teeth of these two genera have been exhibited and sold in London as the teeth of the two sexes of *Buccinum undatum*," the difference between the two being that in *Fusus* the central teeth have three points, while in *Buccinum* they have seven. Whelks' palates, as they are called, are, you know, very common microscopic objects; and on looking over those in the cabinets of my friends, I have found tongues of at least three different genera, all going under this name.

As to difference in the teeth on account of sex, Woodward remarks, on the authority of Mr. Wilton, that in *Buccinum limbosum* the male has seven points to the central teeth, while the female has only six; and, thinking it might be interesting to make some observations on the subject with regard to our common species the *Buccinum undatum*, I took four specimens, two males and two females, and mounted their tongues, as you will see. The result is

as follows :—1st male, central tooth six points ; 2nd male, central tooth five; 1st female, central tooth seven; and 2nd female, central tooth five points : so that I find a male and a female with each only five points, and, on the other hand, a female with seven points, and a male with six. This shows, I think, that in *Buccinum undatum* there is nothing definite as to sex in the number of points, but that, in describing this ribbon, the points of the central teeth must be stated to vary from five to seven in number.

In *Fusus antiquus* I find the toothlets on the central teeth to be either three or four.

I may remark, that the general character of the teeth in these two genera is perfectly similar, with the exception of the number of denticles just mentioned; and I am able to show a series of specimens, including the two, which run regularly up from three to seven toothlets, thus bringing them extremely near together as regards their lingual dentition.

You will be surprised, then, to find that both Gray and Adams make *Fusus* and *Buccinum* to belong not only to different genera, but even to distinct families. I will not venture to say that this is incorrect, but I may remark that the entire internal anatomy of the two agrees very closely.

I can speak, however, with greater confidence on another point, namely, the manner in which these two animals are distributed by the naturalists just mentioned,—*Fusus*, or *Chrysodomus*, as it is now called, being placed in the family *Muricidæ*, and *Buccinum* being made the type of another family, the *Buccinidæ*, in which *Purpura* is also included. Now, taking *Murex erinaceus* as a type of the *Muricidæ*, its anatomical structure, as well as its lingual ribbon, is totally different from *Fusus*, with which they unite it; while, on the other hand, all the species of *Pur-*

pura I have examined agree in structure and type of ribbon with *Murex*; but in their arrangement *Purpura* is placed with *Buccinum*, from which it is anatomically quite distinct. I say, then, that whether or not *Fusus* and *Buccinum* may be sufficiently different in their operculum and other external characters to require their being placed in distinct families, it is evidently wrong to unite *Fusus* with *Murex*, and *Purpura* with *Buccinum*.

In my drawings, the character of the teeth of the *Muricidæ* is shown by *Purpura biserialis* and *Cerastoma Nuttalli*, which may be compared with the teeth of *Buccinum*, also represented. *Nassa*, I may remark, agrees in its type of teeth with *Buccinum* and *Fusus*.

This closes my present communication on the tongues of Mollusca; but as some members may possibly feel inclined to enter upon the inquiry themselves, I think it will not be amiss to add a few remarks on the manner in which they are to be obtained.

First, as to the kinds best worth the trouble of preparation. Whelks, Limpets, and Trochuses should be taken first. Land and freshwater Snails can scarcely be recommended, except as a special study, their tongues being rather more difficult to find, and the teeth so small that they require a high power to show them properly. It would appear from Spallanzani's description of the anatomy of the head of the Snail, that even he did not make out this part, although, in his curious observations on the reproduction of lost parts, he must have carefully dissected more Snails than any other man.

As to preserving the animals till wanted, they should simply be dropped alive into glycerine or alcohol. Glycerine is perhaps best, where only the tongues are wanted; but it leaves the animals very soft, and as it does not harden their mucus at all, they are very slippery and difficult to work upon when so preserved.

Then, as to the apparatus required for dissection. In the first place, all the work is to be done under water, and a common saucer is generally the most convenient vessel to use. No kind of fastening-down or pinning-out of the animal is needed; and in fact it is much better to have it quite free, that you may turn it about any way you wish. The necessary instruments are a needle-point, a pair of fine-pointed scissors, and small forceps; the forceps should have their points slightly turned in towards each other.

A word or two on the lingual apparatus generally, and on its special characters in a few different animals, will conclude what I have to say.

The mode of using the tongue can be easily seen in any of the common Water Snails, when they are crawling on the glass sides of an aquarium: it may then be observed that from between the fleshy lips a thick mass is protruded, with a motion forwards and upwards, and afterwards withdrawn, these movements being almost continually repeated. The action has the appearance of licking; but when the light falls suitably on the protruded structure, it is seen to be armed with a number of bright points, which are the lingual teeth, so arranged as to give the organ the character and action of a rasp.

If you proceed to dissection, and open the head of one of these Mollusca (say, for instance, a common Limpet), you will find the cavity of the mouth almost filled with the thick fleshy mass the front of which is protruded in the act of feeding, and on its upper surface, extending along the middle line, from back to front, is seen the strong membranous band upon which the teeth are set. The mass itself consists of a cartilaginous frame, surrounded by strong muscles; and these structures constitute the whole of the active part of the lingual apparatus. In the Plate of anatomical details you will see the parts represented.

But the peculiarity of the toothed membrane which makes its name of "ribbon" so appropriate is, that there is always a considerable length of it behind the mouth, perfectly formed, and ready to come forward and supply the place of that at the front, which is continually wearing away by use.

In the Limpet this reserve ribbon is of great length, being nearly twice as long as the body, and the whole of it is exposed to view on simply removing the foot of the animal: nothing, then, can be easier than to extract the tongue of the common Limpet. But, unfortunately, what you find in one kind of Mollusk is not at all what you find in another. In the Acmæas for instance, which are very closely related to the Limpets, and have shells which cannot be distinguished, the reserve portion of the ribbon has to be dug out from the substance of the liver, in which it is imbedded, that organ being, as it were, stitched completely through by a long loop of it, as shown in the drawing (Pl. VII.) of one species of this animal.

It might be thought a comfortable reflection, that, at all events, one end of the ribbon can always be found in the mouth; but in many cases this is about the worst place to look for it. Perhaps it may appear strange, but in some of the smaller species, with a retractile trunk, a beginner may very likely fail altogether in his attempt to find the mouth; if, however, the skin of the back is removed, commencing just behind the tentacles, there will be very little difficulty in making out the trunk, which either contains the whole of the ribbon, as in the Whelk, or the front part of it, as in *Purpura* and *Murex*, where a free coil is also seen to hang from its hinder extremity. Examples of these two forms are represented in the drawings (Pl. VII.).

In the Periwinkles the same plan of proceeding, by at once opening the back of the animal, is best; and, on doing

so, the long ribbon, coiled up like a watch-spring, cannot fail to be found.

In the Trochuses, and indeed in all the Scutibranchiata, one point of the scissors should be introduced into the mouth of the animal, and an incision made directly backwards in the middle line above to some distance behind the tentacles; the tongue is then immediately brought into view, lying along the floor of the mouth.

EXPLANATION OF PLATES.

Plate IV.

Cyclobranchiata.

Fig. 1. Patella vulgata.
Fig. 2. Patella pellucida.
Fig. 3. Acmæa pelta.
Fig. 4. Acmæa patina (profile).
Fig. 5. Tecturella grandis.

Plate V.

Scutibranchiata.

Fig. 1. Trochus zizyphinus.
Fig. 2. Haliotis (sp. from California).
Fig. 3. Fissurella nigropunctata.

Plate VI.

Pectinibranchiata.

Fig. 1. Buccinum undatum.
Fig. 2. Purpura biserialis.
Fig. 3. Cerastoma Nuttalli.
Fig. 4. Natica monilifera.
Fig. 5. Luponia vitellus.

Plate VII.

Anatomical details of Lingual Apparatus.

Fig. 1. Patella (American species), with the tongue protruded. (Magnified.)
Fig. 2. Patella vulgata. The head opened above, showing the lingual apparatus. (Magnified.)
Fig. 3. Patella vulgata. The foot removed, showing the reserve ribbon.
Fig. 4. Acmæa (American species), showing a long loop of ribbon on the back.
Fig. 5. Purpura melones, showing the trunk.
Fig. 6. Murex erinaceus, showing the trunk and free coil of ribbon. (Magnified.)
Fig. 7. Buccinum undatum: the trunk partly protruded.

VII.—*On the Influence of the Seasons upon the Rate of Decrease of the Temperature of the Atmosphere with Increase of Height, in different Latitudes in Europe and Asia.* By Joseph Baxendell, Esq., F.R.A.S.

Read December 24th, 1861.

The determination of the laws of the distribution of heat in the different strata of the atmosphere under various circumstances of season, locality, direction of the wind, barometric pressure, &c., is one of the most interesting, and at the same time one of the most difficult problems which can engage the attention of the meteorologist. Notwithstanding the labours of many able meteorologists and physicists, several points of considerable importance to the future progress of meteorology are still involved in doubt and obscurity, and the necessity for further inquiries has been so generally acknowledged that, at the late Meeting of the British Association in this city, a grant of £200 was renewed to defray the expenses of balloon ascents, to be undertaken for the purpose of obtaining additional data of a reliable character to serve as a basis for future investigations. I have therefore thought that it might be worth while to submit to this Society some results which, although confessedly imperfect, seem to me to indicate very clearly the existence of a law of distribution of temperature in the higher regions of the atmosphere in the different seasons, in different latitudes of Europe and Asia, which appears to have hitherto escaped notice, and which seems likely to have an important bearing upon many interesting questions in meteorology.

From numerous observations made at elevated stations in Europe and in India, it has been concluded,—1st, that the general rate of decrease of the temperature of the

atmosphere with increase of height is least in low, and greatest in high latitudes; and 2nd, that the rate of decrease is greatest in the summer, and least in the winter months. Some results, however, which I obtained in the course of an investigation of the relations which exist between falls of rain and changes of barometric pressure, and of the decrement of temperature of the atmosphere in different localities, led me to doubt the general correctness of the second of these conclusions, and I have therefore examined all the observations that were accessible to me that seemed likely to throw any light on the subject; and I have obtained some results which seem to prove the existence of a belt in the temperate latitudes of Europe and Asia, in which the decrease of temperature for a given ascent in the atmosphere is greatest in the winter months, while at stations north or south of this belt, so far at least as observations have yet been made, the decrease is greatest in the summer months.

This belt passes over Portugal, Spain, Sicily, Southern Italy, the Caucasian provinces, and Southern Siberia.

Bywell and Allenheads Stations.

The only trustworthy observations made in England at stations differing considerably in altitude, and not too far apart in a horizontal direction, which I have yet met with, are those made at Bywell and Allenheads in Northumberland, under the direction of Mr. T. Sopwith, F.R.S. The difference of elevation of the two stations is 1273 feet, and the mean temperatures of the winter and summer quarters and the differences are—

	Winter Quarter.	Summer Quarter.
	°	°
Bywell	38·7	58·5
Allenheads	34·9	53·4
Differences	3·8	5·1

If we divide the years into six winter and six summer months, we have the following numbers:—

	Mean of six Winter months.	Mean of six Summer months.
	°	°
Bywell	41·62	54·2
Allenheads	37·24	49·2
	4·38	5·0

These results are from observations made during the five years 1856–1860, and nine months of the year 1861.

Geneva and the Hospice of the Great St. Bernard.

Difference of elevation of the stations = 6838 feet.

	Mean Temp. of Winter Quarter.	Mean Temp. of Summer Quarter.
	°	°
Geneva	32·9	63·1
Great St. Bernard	16·4	41·4
	16·5	21·7

The data for these stations were taken from the tables of monthly mean temperatures for the years 1848–1859, given by Mr. Vernon in his valuable paper "On the Barometric Oscillations at Geneva and the Great St. Bernard, and their Relations to Temperature and the Fall of Rain."

Dijon and Great St. Bernard.

Difference of altitude = 7368 feet.

	Winter Quarter.	Summer Quarter.
	°	°
Dijon	35·4	69·6
Great St. Bernard	16·4	41·4
	19·0	28·2

Milan and Great St. Bernard.

Difference of altitude = 7631 feet.

	Winter Quarter.	Summer Quarter.
	°	°
Milan	35·0	71·9
Great St. Bernard	16·4	41·4
	18·6	30·5

Berne and St. Gothard.

Difference of altitude = 4954 feet.

	Winter Quarter.	Summer Quarter.
	°	°
Berne	30·4	60·4
St. Gothard	18·3	44·1
	12·1	16·3

Milan and St. Gothard.

Difference of altitude = 6390 feet.

	°	°
Milan	35·0	71·9
St. Gothard	18·3	44·1
	16·7	27·8

Venice and Hohe-Peissenberg (47° 48′ N., 11° 1′ E.).

Difference of altitude = 3146 feet.

	°	°
Venice	38·4	72·5
Hohe-Peissenberg	29·1	57·9
	9·3	14·6

Munich and Hohe-Peissenberg.

Difference of elevation = 1491 feet.

	°	°
Munich	28·7	61·6
Hohe-Peissenberg	29·1	57·9
	−0·4	3·7

Vienna and Munich.

Difference of elevation = 1228 feet.

	°	°
Vienna	31·9	69·4
Munich	28·7	61·6
	3·2	7·8

Passing now to stations in and near the Ural Mountains, we find that most of these stations are too far apart, and the differences of elevation too small, to enable us to arrive at any very decisive result: taking, however, the highest

station, Zlatouste, 55° 8′ N., 59° 28′ E., at an elevation of 1230 feet above the sea, and the two most suitable stations for comparison with it, Ufa, 54° 42′ N., 55° 59′ E., and 500 feet above sea-level, and Kourgan, 55° 20′ N., 65° 0′ E., and also about 500 feet above the level of the sea, we have these results:—

	Winter.	Summer.
	°	°
Ufa	12·0	67·3
Kourgan	1·6	67·1
Mean of Ufa and Kourgan	6·8	67·2
Zlatouste	3·9	58·8
	2·9	8·4

These numbers, like those we have obtained for places in Central and Western Europe, show that the greatest difference occurs in the summer months; and we may therefore fairly conclude that the same law holds good over the whole breadth of the Continent.

All the stations at which meteorological observations have been made in Northern Siberia, are too nearly on the same level to afford data to enable us to pursue the inquiry in that distant region, and I shall therefore now pass to stations in India.

Madras and Dodabetta.

Difference of elevation = 8600 feet.

As the observations at Dodabetta were only taken twice a day, I have taken the means for Madras at the same hours. The comparison will therefore be,—

From observations at 9^{h} 40^{m} A.M.

	Winter Quarter.	Summer Quarter.
	°	°
Madras	79·6	89·2
Dodabetta	51·1	52·7
	28·5	36·5

From observations at $3^h\ 40^m$ P.M.

	Winter.	Summer.
	°	°
Madras	81·7	93·0
Dodabetta	51·7	53·2
	30·0	39·8

Dividing the year into six winter and six summer months, we have the following comparisons:—

From observations at $9^h\ 40^m$ A.M.

	Winter.	Summer.	
Madras, less Dodabetta	29°·6	35°·4	Diff. = 5°·8

From observations at $3^h\ 40^m$ P.M.

	Winter.	Summer.	
Madras, less Dodabetta	31°·3	37°·8	Diff. = 6°·5

As the difference between the winter and summer temperatures at Bombay differs only *four-tenths* of a degree from that at Madras, a comparison of Bombay with Dodabetta would lead to almost identical results; and the summer differences being greater than the winter, it is evident that the law which exists in the middle latitudes of Europe holds good also in the low latitudes of Southern India.

The observations to which I have as yet had access from stations in Northern India are mostly for very short periods; and as the stations are inconveniently far apart, and the differences of altitude between them generally small, the results obtained have been rather discordant, and therefore render it impossible to determine at present, even approximately, the parallel of latitude at which the summer difference of temperature ceases to be greater than that for winter; and I will therefore now proceed to give the details for the stations in Europe and Asia at

which the difference of temperature is greatest in the winter months.

The highest station in South-western Europe at which observations have been made is Madrid, and the low stations nearest to it in the direction of a parallel of latitude are Lisbon to the West, and Barcelona to the East.

Lisbon and Madrid.

Difference of altitude = 1780 feet.

	Winter.	Summer.
	°	°
Lisbon	52·5	70·9
Madrid	44·4	74·3
	8·1	−3·4

Barcelona and Madrid.

Difference of altitude = 2000 feet.

	°	°
Barcelona	50·0	76·1
Madrid	44·4	74·3
	5·6	1·8

In Spain, therefore, the difference of temperature between the higher and lower strata of the atmosphere is greater in winter than in summer.

The next stations are in Sicily.

Messina and Nicolosi near Catania.

Difference of altitude = 2300 feet.

	°	°
Messina	55·0	77·2
Nicolosi	51·2	78·6
	3·8	−1·4

Catania and Nicolosi.

Difference of altitude = 2300 feet.

	°	°
Catania	54·7	80·4
Nicolosi	51·2	78·6
	3·5	1·8

Kaemtz gives the mean temperatures of the seasons at a station called the Casino on Mount Etna, at an elevation

of 9809 feet above the sea, but does not state the number of years from which his results are derived. His results, however, compared with those given above for Messina and Catania, agree in showing a greater difference of temperature between the high and the low stations in winter than in the summer months.

Proceeding again in an easterly direction, we arrive next at the mountainous region lying between the Black Sea and the Caspian, where we find several important stations well suited for our pupose, at which for some years meteorological observations have been made by direction of the Russian Government. These stations are—

	°	′	°	′	
Tiflis	41	42 N.	44	50 E.	1500 feet above sea-level.
Koutaïs	42	31	42	47	470 ,, ,,
Aralikh	39	53	44	38	2600 ,, ,,
Alexandropol	40	47	43	47	4800 ,, ,,
Bacou	40	22	49	50	53 feet below sea-level.
Lenkoran	38	44	48	53	65 ,, ,,
Alagir	43	5	44	19	2060 feet above sea-level.

Koutaïs and Alagir.

Difference of altitude = 1590 feet.

	Winter.	Summer.
	°	°
Koutaïs	41·2	73·2
Alagir	27·0	66·1
	14·2	7·1

Koutaïs and Tiflis.

Difference of altitude = 1030 feet.

	°	°
Koutaïs	41·2	73·2
Tiflis	34·5	73·9
	6·7	−0·7

Koutaïs and Aralikh.

Difference of altitude = 2130 feet.

	°	°
Koutaïs	41·2	73·2
Aralikh	25·9	76·6
	15·3	−3·4

Koutaïs and Alexandropol.

Difference of altitude = 4330 feet.

	Winter.	Summer.
	°	°
Koutaïs	41·2	73·2
Alexandropol	18·8	64·3
	22·4	8·9

Lenkoran and Aralikh.

Difference of altitude = 2665 feet.

	°	°
Lenkoran	41·9	75·5
Aralikh	25·9	76·6
	16·0	−1·1

Bacou and Tiflis.

Difference of altitude = 1553 feet.

	°	°
Bacou	40·0	76·2
Tiflis	34·5	73·9
	5·5	2·3

Bacou and Aralikh.

Difference of altitude = 2653 feet.

	°	°
Bacou	40·0	76·2
Aralikh	25·9	76·6
	14·1	−0·4

Bacou and Alexandropol.

Difference of altitude = 4853 feet.

	°	°
Bacou	40·0	76·2
Alexandropol	18·8	64·3
	21·2	11·9

Tiflis and Alexandropol.

Difference of altitude = 3300 feet.

	°	°
Tiflis	34·5	73·9
Alexandropol	18·8	64·3
	15·7	9·6

Alagir and Alexandropol.

Difference of altitude = 2740 feet.

	Winter.	Summer.
	°	°
Alagir	27·0	66·1
Alexandropol	18·8	64·3
	8·2	1·8

The results thus given for stations in the Caucasian and Transcaucasian provinces show in a very marked manner that in this region the diminution of temperature for a given ascent in the atmosphere is greater in the winter than in the summer months.

We now proceed to three stations in Southern Siberia.

Irkoutzk	52°	17′ N.	104°	11′ E.	1253 feet above sea-level.
Werchne-Udinsk	51	30	107	44	1970 " "
Nertchinsk	51	18	119	21	2230 " "

Irkoutzk and Werchne-Udinsk.

Difference of altitude = 717 feet.

	°	°
Irkoutzk	−1·3	61·4
Werchne-Udinsk	−2·2	65·0
	0·9	−3·6

Irkoutzk and Nertchinsk.

Difference of altitude = 977 feet.

	°	°
Irkoutzk	− 1·3	61·4
Nertchinsk	−16·6	60·8
	15·3	0·6

With respect to the latter result, it must be remarked that a great portion of the difference belongs undoubtedly to the difference of longitude, and not to the difference of elevation between the two stations. In this part of Asia, the differences between summer and winter temperatures increase rapidly with increase of longitude, and tend to complicate the inquiry into the influence of difference of elevation.

No observations have been made at elevated stations in Eastern Asia; and therefore I have been unable to pursue my inquiries beyond the station last given, Nertchinsk.

I do not intend to enter, in the present paper, into a consideration of the probable causes of the phenomenon which I have endeavoured to bring under the notice of the Society. My principal motive for bringing the subject forward in its present imperfect state is the hope that it may induce others, having greater facilities of access to good observations than I myself possess, to enter into the inquiry, and endeavour to trace out, more accurately than I have the means of doing, the exact direction and limits of the remarkable belt indicated by my results. I may, however, remark that, if the great changes of temperature which take place in the higher strata of the atmosphere in this belt are produced by the direct action of the sun's rays, it would indicate a less capacity for heat of the air in these elevated strata than of that in corresponding strata beyond the belt. Such a diminished capacity for heat would be produced by a diminution in the quantity of moisture contained in the air; and we might therefore expect that the ratio of the quantities of rain falling at two stations, one of which is considerably elevated above the other, would be sensibly less at places in this belt than in other localities. It will be seen that this view is strongly supported by the following comparisons of the only trustworthy data I have yet been able to obtain:—

Stations North and South of the Belt.

1. *Allenheads and Bywell.*

	inches.
Mean annual rainfall at Allenheads, from five years' observations	=45·26
Mean annual rainfall at Bywell, from five years' observations	=24·90

The ratio is therefore =1·81.

2. *Great St. Bernard and Geneva.*

	inches.
Mean annual rainfall, from eleven years' observations, at Great St. Bernard	=45·64
Mean annual rainfall, from eleven years' observations, at Geneva........	=31·61

The ratio is therefore =1·44

3. *Dodabetta and Madras.*

Mean annual rainfall, from eight years' observations, at Dodabetta	=84·72
Mean annual rainfall, from eight years' observations, at Madras........	=56·04

The ratio is therefore =1·51

4. *Dodabetta and Bombay.*

Mean annual rainfall, from eight years' observations, at Dodabetta	=84·72
Mean annual rainfall, from eight years' observations, at Bombay	=73·40

The ratio is therefore =1·15

Stations in the Belt.

1. *Madrid and Lisbon.*

Mean annual rainfall, from five years' observations, at Madrid	= 9·86
Mean annual rainfall, from five years' observations, at Lisbon	=30·68

The ratio is therefore only =0·32

2. *Alagir and Koutaïs.*

Mean annual rainfall, from four and a half years' observations, at Alagir	=37·67
Mean annual rainfall, from four and a half years' observations, at Koutaïs........	=57·94

The ratio is therefore =0·65

3. *Alexandropol and Lenkoran.*

Mean annual rainfall, from six years' observations, at Alexandropol	=15·75
Mean annual rainfall, from four years' observations, at Lenkoran........	=49·48

The ratio is therefore =0·31

Grouping together the *four* highest and the *three* lowest

stations in the Caucasian provinces, we have the following comparisons :—

Four highest Stations.

	Height.	Mean annual rainfall.
Aralikh...............	2600 feet	6·07 inches
Alexandropol	4800 „	15·75 „
Alagir	2060 „	37·67 „
Tiflis	1500 „	17·27 „
Mean	2740	19·19

Three lowest Stations.

Koutaïs...............	470 feet	57·94 inches
Bacou	−53 „	11·38 „
Lenkoran	−65 „	49·48 „
Mean	117	39·60

The ratio of the mean quantities =0·48.

From the results thus given, it will be observed that, at all the stations lying outside the belt, the ratio is greater than unity, the fall of rain on the mountain exceeding that on the plain; while at stations within the belt the ratio is less than unity, the fall on the plain being greater than that on the mountain. We see therefore that, with reference to the rainfall, there is an inversion of phenomena in the belt, similar to that which takes place with regard to the changes of the decrement of temperature with increase of altitude,—and also that, on the average of the year, the rain-producing stratum of the air is relatively of less depth at stations within the belt than at other places—or, perhaps to speak more correctly, that moisture in a state fit for the immediate production of rain is relatively less abundant in the higher strata of the atmosphere in the belt than in the corresponding strata on either side.

If we compare the ratios of the winter half of the year with those for the summer half at the different stations, we find that a similar relation holds good.

1. *Stations beyond the Belt.*

	Winter ratio.	Summer ratio.
Allenheads and Bywell	$\frac{23.88}{13.40}=1.78$	$\frac{21.38}{15.50}=1.37$
Great St. Bernard and Geneva	$\frac{22.015}{12.207}=1.80$	$\frac{23.625}{19.406}=1.21$
Dodabetta and mean of Madras and Bombay	$\frac{27.200}{16.220}=1.67$	$\frac{57.520}{48.497}=1.18$

At all these stations, therefore, the ratio is greater in the *winter* than in the summer half of the year.

2. *Stations in the Belt.*

	Winter ratio.	Summer ratio.
Madrid and Lisbon	$\frac{5.526}{23.453}=0.235$	$\frac{4.330}{7.228}=0.559$
Alagir and Koutaïs	$\frac{9.215}{33.426}=0.275$	$\frac{28.452}{24.518}=1.16$
Alexandropol and Lenkoran	$\frac{5.951}{31.196}=0.190$	$\frac{9.084}{18.285}=0.536$

We see, then, that at stations within the belt the ratio is greatest in the *summer* half of the year; and it would therefore appear that at these stations the quantity of rain-forming moisture in the higher strata of the air, as compared with that in the lower, is relatively greater in the summer than in the winter half of the year; while, on the contrary, at stations beyond the belt it is greatest in the winter half.

Before concluding this paper, I may take the opportunity of drawing attention to some results which appear to indicate a periodical change in the annual value of the rate of decrease of temperature for a given ascent in the atmosphere. In the following Table I have given the mean annual temperatures, and the differences, for the two stations Geneva and the Hospice on the Great St. Bernard, for the years 1848–1858 :—

	Geneva. Mean temp.	Gt. St. Bernard. Mean temp.	Diff.
	°	°	°
1848	47·75	28·85	18·90
1849	48·08	28·85	19·23
1850	47·26	27·77	19·49
1851	46·17	27·21	18·96
1852	48·99	29·30	19·69
1853	47·28	26·73	20·55
1854	48·18	28·11	20·07
1855	47·70	27·52	20·18
1856	48·29	28·48	19·81
1857	48·77	28·78	19·99
1858	47·95	28·46	19·49

From the numbers in the last column, we see that the difference was at a maximum in 1853, and at a minimum in 1848, and that, notwithstanding some slight irregularities, there was a tolerably regular increase from 1848 to 1853, and afterwards a tolerably regular decrease to 1858. As it is generally believed that the temperatures at low stations are more liable to be affected by accidental irregularities than those of stations at a greater elevation, I have also compared Milan with the Great St. Bernard, and have obtained the following differences:—

	°		°
1848	25·35	1854	26·84
1849	26·68	1855	26·39
1850	25·85	1856	25·71
1851	26·73	1857	25·65
1852	25·76	1858	24·79
1853	27·76		

These differences also show a maximum in 1853, but the minimum is in 1858. A slight examination will, however, show that the irregularities in the first series are to some extent compensated by the irregularities in the second; and combining the two, we have—

	°		°
1848	22·12	1854	23·45
1849	22·95	1855	23·28
1850	22·67	1856	22·76
1851	22·84	1857	22·82
1852	22·72	1858	22·14
1853	24·15		

If, now, in order to smooth down still further the irregularities arising from accidental causes at both the upper and the lower stations, we take the means of groups of three years, we have the following remarkable numbers:—

Mean of	1848–50	=22°·58
„	1849–51	=22·80
„	1850–52	=22·74
„	1851–53	=23·23
„	1852–54	=23·44
„	1853–55	=23·62
„	1854–56	=23·16
„	1855–57	=22·95
„	1856–58	=22·57

In looking over these numbers, it seems impossible to resist the conclusion that some influence has been in operation by which the temperature of the higher station was gradually reduced, as compared with the lower stations, up to the beginning of the year 1854, and afterwards as gradually increased to the close of the series.

In the next Table I have given the mean annual temperatures and the differences for the years 1856–1860 at the two stations in England, Bywell and Allenheads:—

	Bywell. Mean ann. temp.	Allenheads. Mean ann. temp.	Diff.
1856	46°·92	42°·78	4°·14
1857	49·50	44·82	4·68
1858	48·47	43·57	4·90
1859	48·81	43·90	4·91
1860	45·73	40·66	5·07

Here we have a remarkably gradual and regular increase of the difference from the commencement to the end of the series, which contrasts strongly with the irregularities in the changes of the actual temperatures of the two stations; and it will be noticed that this gradual *increase* took place in the years during which a *decrease* occurred in the difference between the Milan and Geneva and Great St. Bernard Stations.

It is remarkable that the epoch of maximum indicated by the Geneva and Great St. Bernard observations corresponds exactly with the epoch of minimum magnetic disturbance, as determined by General Sabine from the observations made at the Colonial observatories and at Pekin; and it is probable that there is also a close correspondence between the *periods* of the two phenomena.

Mr. Vernon, in his paper "On the Irregular Barometric Observations at Geneva and the Great St. Bernard," has given the mean monthly temperatures at these two stations, derived from observations made during the twenty years 1836–1855; and Prof. Plantamour, in a paper in the 13th volume of the 'Memoirs of the Physical and Natural History Society of Geneva,' has given the mean annual temperatures from observations during the ten years 1841–1850. From these data, and those given above, I find that—

The average difference of temperature of the two stations for the five years	 1836–40	= 19°·046
" " seven years	 1841–47	= 19·697
" " three years	 1848–50	= 19·206
" " seven years	 1851–57	= 19·890

These results, taken in connexion with the course of the curve laid down from the numbers derived from the comparison of Geneva and Milan with the Great St. Bernard, indicate a period of about ten years, which, according to General Sabine, is also the period of magnetic disturbance.

I cannot conclude without expressing my grateful acknowledgments to my friend Mr. Vernon, F.R.A.S., for the valuable assistance he has rendered me in procuring data, and in referring to original publications for the purpose of clearing up doubtful points; and I may add that, without the means of reference afforded by the many valuable volumes of meteorological observations now in the Society's Library, it would have been quite impossible to have undertaken an inquiry of this nature.

VIII.—*On the Direction of the Wind at Manchester, during the years* 1849–1861, *at* 8^h A.M. By G. V. VERNON, Esq., F.R.A.S., M.B.M.S.

Read before the Physical and Mathematical Section, February 6th, 1862.

THE direction of the wind has been referred to sixteen points of the compass in the observations, but only eight have been used in this investigation, this being considered quite sufficient for the object in view.

In January, the wind which blows upon the largest number of days is the S.W., and the least prevailing winds the N. and N.E., the former occurring upon the least number of days.

In February the S.W. wind is still the most frequent, but blows on a less number of days than in January: the least frequent winds are the N. and W.; the former, however, shows an increase on January, and the latter a decrease.

In March the most prevalent wind is still the S.W., and the least prevalent the N.; the N.W. wind also occurs upon more days than either in January or February, having progressively increased each month: E. winds also occur on rather more days than in either of the two preceding months: S. winds also show a diminution from the beginning of the year.

In April we find the most prevalent wind to be the N.E., and the least the N., as in the preceding months; there is also a large increase in E. winds. The S.W. winds come next to the N.E. in amount, but occur on fewer days than in March: this wind has diminished from the beginning of the year, the monthly figures being 8·3, 7·6, 6·3, and 5·1 respectively. The N.W. winds, which gradually increase in amount in the preceding months,

appear to receive a check this month, and there is a slight falling off in the amount.

In May the most prevalent wind is the N.E., being almost identical with the preceding month; the E. winds also about the same. The S.W. wind shows an increase this month as compared with April. The N. winds also show an increase.

In June the most prevalent wind is the S.W., showing an increase of more than 50 per cent. on the month of May: the least prevalent wind is the N., which appears to have blown upon six days only in this month during thirteen years. N.W. winds show an increase of 50 per cent. over May; N.E. and E. winds are nearly 50 per cent. below what they were in May.

In July the most prevalent winds are the S.W., but to a less extent than in June; next come the N.W., which show an increase on June: N. winds show an increase, N.E. and E. a diminution, as compared with June, more especially the former.

In August the S.W. winds increase nearly 50 per cent. on what they were in July; N.W. winds fall off suddenly 50 per cent.; W. winds show a slight increase, having progressively advanced each month from April.

In September the most prevailing wind is still the S.W., but showing a decrease of 45 per cent. as compared with August. The least prevailing wind is the N., the thirteen years giving only nine days for this wind. The N.E. winds show an increase of 60 per cent., E. winds an increase of 70 per cent., S.E. winds an increase of 130 per cent., S. winds an increase of 20 per cent., W. winds a slight falling off, and N.W. winds an increase of 25 per cent. as compared with August.

In October S.W. winds are still the most frequent, and show an increase of nearly 25 per cent. on the preceding month. The least prevalent wind is the N.

There is also an increase of rather more than 33 per cent. in S. winds, as compared with September.

In November the S.W. wind is the most prevalent, but has fallen to below the amount for September. The least prevalent wind is the N., but shows an increase of about 62 per cent. on the preceding month: N.E. winds show an increase of 90 per cent., S.E. winds an increase of 14 per cent., and S. winds a falling off of 25 per cent.

In December the S.W. wind still prevails the most, and shows an increase of 17 per cent. The least prevalent wind is the N., but shows an increase of 30 per cent. on the amount for November; N.E. winds show a falling off of about 33 per cent., E. winds a falling off of 12 per cent., S. winds an increase of 35 per cent., W. winds a slight decrease, and N.W. winds an increase of 20 per cent.

In one of the Tables subjoined, the number of days that each wind has blown in each year have been tabulated, and we find that the S.W. blows upon the largest number of days, viz. 91·5 per year, or on 1190 days in the thirteen years. The N. wind blows upon the least number of days, the average being 16·3, and the total number 212. The order of frequency of occurrence of each wind is as follows, commencing with the highest number :—

S.W.	91·5
N.W.	56·0
N.E.	48·6
W.	42·0
S.	40·7
S.E.	38·7
E.	30·3
N.	16·3

These figures do not seem to exhibit any uniformity whatever; and, in order to eliminate still further any

irregularities, I have referred them to the four principal points only, assuming that—

$$N. = \tfrac{1}{2}\ N.E. + N. + \tfrac{1}{2}\ N.W.$$
$$E. = \tfrac{1}{2}\ N.E. + E. + \tfrac{1}{2}\ S.E.$$
$$S. = \tfrac{1}{2}\ S.E. + S. + \tfrac{1}{2}\ S.W.$$
$$W. = \tfrac{1}{2}\ S.W. + W. + \tfrac{1}{2}\ N.W.$$

The means of all the years then give

$$N. = 68{\cdot}6\ ;\ E. = 74{\cdot}0\ ;\ S = 105{\cdot}8\ ;\ W. = 115{\cdot}8,$$

showing a gradual increase towards the W. The mean yearly direction was S. 45° W., or exactly S.W.

The maximum variation towards the W. occurred in 1857, and was S. 74° W., the minimum in 1861, and was S. 31° W., so that the range of the mean direction was 43°.

In the diagrams or wind-roses (Pls. VIII. & IX.), the mean amount of each wind, referred to each month separately, can be seen at a glance; and the relative areas of the various diagrams also show the frequency of the occurrence of each wind during the year. All these diagrams have been drawn to scale.

In Table III. the amounts of each wind are given for each meteorological season. We see in this Table that S.W. winds predominate in the winter months, and, moreover, blow upon more days in this quarter than in any of the remaining quarters.

In the spring quarter the N.E. and S.W. predominate, and are nearly equal: the N.E. wind blows upon more days this quarter than in any other quarter.

In the summer quarter the S.W. wind blows upon the greatest number of days; next comes the N.W. wind, which has a greater prevalence this quarter than in any of the other three quarters.

In the autumn quarter the S.W. is the most prevalent wind; then come the W., S., S.E., N.E., and N.W., which

are nearly equal; the N.W. is at its minimum amount this quarter.

When the winds are referred to the four principal points only, we find the great prevalence of the S. wind in winter very marked: this wind blows upon more days in the winter quarter than any of the remaining winds referred to each separate quarter, with one exception, viz. the W. wind, which slightly exceeds it in the summer quarter.

The E. winds of spring become also very prominent when referred to the four cardinal points only, and greatly exceed the amounts in the other three seasons. With the summer comes the great excess of W. winds, and diminution of E. winds. Autumn brings an increase of N. and E. winds, and a falling off of W. and S. winds.

The distribution of the winds during the year is of very great importance, especially as bearing upon the question of the public health. Examination of the returns of the Registrar-General shows very distinctly the very large increase which takes place in deaths from Phthisis during that period of the year in which the E. wind prevails. When the deaths from Phthisis, on an average of ten years for London, are laid down in a curve, the maximum amounts of mortality from this disease occur in the spring—precisely the period in which easterly winds so greatly prevail.

Table I.—Direction of Wind at Manchester, 8 a.m.

January.

Year.	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.
1849.	0	6	0	1	6	12	2	4
1850.	2	3	8	11	1	5	0	1
1851.	1	1	0	8	10	4	7	0
1852.	0	2	0	1	2	14	1	11
1853.	0	3	2	3	1	11	3	8
1854.	0	7	2	4	4	14	0	0
1855.	0	12	1	1	0	7	0	10
1856.	1	11	1	6	5	4	1	2
1857.	3	9	0	5	0	8	1	5
1858.	3	2	0	4	9	6	4	3
1859.	1	2	0	0	4	16	5	3
1860.	3	2	4	4	8	3	5	2
1861.	2	4	6	5	7	4	3	0
Sums ...	16	64	24	53	57	108	32	49
Means...	1·2	4·9	1·8	4·1	4·4	8·3	2·4	3·8

February.

Year.	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.
1849.	1	1	0	4	2	17	2	1
1850.	0	0	0	3	4	17	2	2
1851.	2	2	5	2	10	3	2	2
1852.	2	5	0	1	2	10	0	9
1853.	6	6	0	2	2	0	1	11
1854.	3	1	0	0	2	10	4	8
1855.	1	19	4	1	1	1	0	1
1856.	1	5	0	3	3	10	0	7
1857.	1	0	2	4	5	10	4	2
1858.	1	2	10	9	2	2	0	2
1859.	0	0	0	8	2	12	4	2
1860.	7	2	2	0	4	2	5	7
1861.	0	2	5	4	11	5	1	0
Sums ...	25	45	28	41	50	99	25	54
Means...	1·9	3·5	2·1	3·2	3·9	7·6	1·9	4·2

TABLE I. (*continued*).

March.

Year.	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.
1849.	7	3	1	2	0	8	1	9
1850.	2	6	3	0	5	10	2	3
1851.	0	1	3	5	3	8	7	4
1852.	1	12	3	7	0	4	2	2
1853.	3	7	0	5	2	5	1	8
1854.	0	4	1	2	3	7	6	8
1855.	3	10	0	5	1	5	2	5
1856.	2	8	10	2	2	1	1	5
1857.	2	5	6	6	1	4	4	3
1858.	1	3	4	1	0	6	10	6
1859.	1	0	0	1	3	12	5	9
1860.	0	0	2	3	9	3	11	3
1861.	2	1	0	2	3	9	9	5
Sums ...	24	60	33	41	32	82	61	70
Means...	1·8	4·6	2·5	3·2	2·4	6·3	4·7	5·4

April.

Year.	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.
1849.	5	3	0	10	0	9	0	3
1850.	1	4	1	7	8	6	2	1
1851.	3	7	4	3	5	6	1	1
1852.	0	9	4	8	1	7	0	1
1853.	0	3	2	0	2	6	3	14
1854.	2	6	8	0	1	2	3	8
1855.	0	11	0	1	0	2	4	12
1856.	0	7	4	3	4	5	1	6
1857.	0	7	2	6	5	5	3	2
1858.	2	3	9	0	3	5	2	6
1859.	0	2	6	2	3	5	4	8
1860.	2	9	6	2	3	4	3	1
1861.	2	13	3	0	1	4	7	0
Sums ...	17	84	49	42	36	66	33	63
Means...	1·3	6·5	3·8	3·2	2·8	5·1	2·5	4·9

TABLE I. (*continued*).

May.								
Year.	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.
1849.	6	6	0	6	3	7	0	3
1850.	0	1	4	2	6	7	5	6
1851.	2	2	3	1	1	6	9	7
1852.	4	10	3	0	1	8	1	4
1853.	0	14	3	4	0	1	1	8
1854.	1	4	0	4	2	13	4	3
1855.	0	12	2	5	1	7	0	4
1856.	1	14	2	2	1	9	1	1
1857.	0	8	9	3	1	4	5	1
1858.	2	1	3	2	4	10	2	7
1859.	1	6	13	7	1	0	1	2
1860.	1	1	5	2	11	4	4	3
1861.	5	4	1	0	1	5	9	6
Sums ...	23	83	48	38	33	81	42	55
Means...	1·8	6·4	3·7	3·0	2·5	6·2	3·2	4·2

June.								
Year.	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.
1849	2	3	0	3	3	16	0	3
1850	0	2	1	0	10	10	7	0
1851	0	0	1	3	1	15	5	5
1852	0	6	2	3	0	12	0	7
1853	1	7	0	0	0	9	4	9
1854	1	7	0	2	1	11	6	2
1855	0	2	0	1	2	16	0	9
1856	0	1	1	1	1	6	9	11
1857	1	6	4	2	5	4	2	6
1858	0	0	0	4	3	11	1	11
1859	0	4	8	2	6	0	2	8
1860	0	6	1	1	5	11	4	2
1861	1	1	6	4	3	2	2	11
Sums...	6	45	24	26	40	123	42	84
Means...	0·5	3·5	1·8	2·0	3·1	9·5	3·2	6·5

TABLE I. (*continued*).

July.

Year.	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.
1849.	2	4	3	1	4	16	1	0
1850.	3	1	1	7	0	5	10	4
1851.	2	5	1	1	1	7	6	8
1852.	5	4	0	4	0	9	1	8
1853.	1	1	0	3	0	14	3	9
1854.	0	4	2	1	1	14	3	6
1855.	0	1	0	5	0	10	2	13
1856.	0	0	0	0	0	10	6	15
1857.	0	0	0	0	4	7	12	8
1858.	2	2	0	3	3	7	2	12
1859.	2	0	5	1	3	2	6	12
1860.	1	1	6	4	5	7	4	3
1861.	2	1	0	5	10	8	4	1
Sums ...	20	24	18	35	31	116	60	99
Means...	1·5	1·8	1·4	2·7	2·4	8·9	4·6	7·6

August.

Year.	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.
1849.	0	2	0	3	1	17	2	6
1850.	3	2	0	1	1	16	5	3
1851.	5	7	1	0	3	8	6	1
1852.	4	1	1	2	4	15	0	4
1853.	4	7	0	3	3	13	1	0
1854.	2	0	0	0	4	19	1	5
1855.	0	1	0	1	2	15	5	7
1856.	0	6	1	5	4	10	3	2
1857.	1	4	7	2	1	3	9	4
1858.	0	2	3	4	2	3	7	10
1859.	1	0	4	0	6	7	9	4
1860.	2	1	1	1	5	16	4	1
1861.	0	0	0	0	2	14	15	0
Sums ...	22	33	18	22	38	156	67	47
Means...	1·7	2·5	1·4	1·7	2·9	12·0	5·2	3·6

TABLE I. (*continued*).

September.								
Year.	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.
1849.	1	8	7	3	2	5	2	2
1850.	1	1	6	10	4	5	2	1
1851.	0	7	2	2	2	4	4	9
1852.	0	8	1	5	0	4	0	12
1853.	0	9	3	1	0	7	5	5
1854.	2	0	0	2	7	11	0	8
1855.	0	10	0	5	0	4	0	11
1856.	3	1	1	4	5	8	5	3
1857.	0	2	3	4	2	12	7	0
1858.	0	1	2	8	6	5	6	1
1859.	0	4	1	4	4	7	8	2
1860.	2	1	5	3	6	3	8	2
1861.	0	0	0	0	7	12	9	2
Sums ...	9	52	31	51	45	87	56	58
Means...	0·7	4·0	2·4	3·9	3·5	6·7	4·3	4·5

October.								
Year.	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.
1849.	1	5	6	1	9	4	3	1
1850.	4	2	2	1	5	5	11	1
1851.	0	1	0	3	7	14	4	2
1852.	1	3	4	3	0	7	2	11
1853.	1	3	0	7	2	17	1	0
1854.	0	4	0	0	3	10	0	14
1855.	0	8	0	3	0	13	2	5
1856.	0	5	4	4	13	1	4	0
1857.	0	1	6	6	8	4	3	3
1858.	0	1	6	7	2	9	6	0
1859.	1	0	9	2	6	9	4	0
1860.	3	0	0	0	5	6	15	2
1861.	0	1	7	9	2	7	4	1
Sums ...	11	34	44	46	62	106	59	40
Means...	0·8	2·6	3·4	3·5	4·8	8·1	4·5	3·0

TABLE I. (*continued*).

November.								
Year.	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.
1849.	0	0	0	9	5	9	5	2
1850.	4	0	0	3	3	13	7	0
1851.	3	8	1	4	2	6	0	6
1852.	0	6	1	3	4	12	1	3
1853.	0	3	2	3	3	7	1	11
1854.	0	7	0	2	2	6	2	11
1855.	0	18	0	3	0	6	0	3
1856.	4	6	4	3	2	2	5	4
1857.	0	1	10	7	6	0	5	1
1858.	1	5	10	4	2	4	2	2
1859.	3	1	7	4	3	6	4	1
1860.	1	8	6	6	3	1	5	0
1861.	1	0	0	2	9	9	3	6
Sums...	17	63	41	53	44	81	40	50
Means...	1·3	4·9	3·2	4·1	3·4	6·2	3·0	3·9

December.								
Year.	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.
1849.	2	3	4	9	0	7	0	6
1850.	1	2	1	6	13	6	1	1
1851.	9	4	1	5	0	4	3	5
1852.	0	0	3	5	3	14	2	4
1853.	2	17	3	3	0	4	2	0
1854.	1	0	0	0	1	7	8	14
1855.	0	7	0	2	6	7	0	9
1856.	0	4	2	4	2	6	4	9
1857.	0	1	0	0	6	22	2	0
1858.	0	0	1	2	15	8	1	4
1859.	3	0	5	5	7	5	3	3
1860.	2	5	10	5	2	2	3	2
1861.	2	1	6	9	6	3	1	3
Sums ...	22	44	36	55	61	95	30	60
Means...	1·7	3·4	2·8	4·2	4·7	7·3	2·3	4·6

TABLE II.

Total amounts of each Wind for the entire Year.

Year.	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.
1849.	27	44	21	52	36	127	18	40
1850.	21	24	27	51	60	105	54	23
1851.	27	45	22	37	45	85	54	50
1852.	17	66	22	42	17	116	10	76
1853.	18	80	15	34	15	94	26	83
1854.	12	44	13	17	31	124	37	87
1855.	4	111	7	33	13	93	15	89
1856.	12	68	30	37	42	72	40	65
1857.	8	44	49	45	44	83	57	35
1858.	12	22	48	48	51	76	43	64
1859.	13	19	58	36	48	81	55	54
1860.	24	36	48	31	66	62	71	28
1861.	17	29	34	40	62	72	67	35
Sums ...	212	632	394	503	530	1190	547	729
Means...	16·3	48·6	30·3	38·7	40·7	91·5	42·0	56·0

Referred to Four Points only.					
Year.	N.	E.	S.	W.	Mean direction for year.
1849.	69·0	69·0	125·5	101·5	S. 40° W.
1850.	44·5	64·5	138·0	118·0	S. 34° W.
1851.	74·5	63·0	106·0	121·5	S. 50° W.
1852.	88·0	76·0	96·0	106·0	S. 50° W.
1853.	99·5	72·0	79·0	114·5	S. 60° W.
1854.	77·5	43·5	101·5	142·5	S. 62° W.
1855.	104·0	79·0	76·0	106·0	S. 57° W.
1856.	78·5	82·5	96·5	108·5	S. 45° W.
1857.	47·5	93·5	108·0	116·0	S. 74° W.
1858.	55·0	83·0	113·0	113·0	S. 34° W.
1859.	49·5	85·5	106·5	122·5	S. 34° W.
1860.	56·0	81·5	112·5	116·0	S. 36° W.
1861.	49·0	68·5	118·0	120·5	S. 31° W.
Sums ...	892·0	961·5	1376·5	1504·5	
Means...	68·6	74·0	105·8	115·8	S. 45° W.

TABLE III.—Days of each Direction of Wind in each Season.

Point.	Winter Quarter. Dec., Jan., Feb.	Spring Quarter. March, April, May.	Summer Quarter. June, July, August.	Autumn Quarter. Sept., Oct., Nov.
N.......	4·8	4·9	3·7	2·8
N.E. ...	11·9	17·5	7·8	11·5
E.......	6·8	10·0	4·6	9·0
S.E. ...	11·5	9·4	6·4	11·5
S.......	13·0	7·7	8·4	11·7
S.W....	23·2	17·6	22·4	21·0
W. ...	6·7	10·4	13·0	11·8
N.W....	12·5	14·5	17·7	11·4

Four Principal Directions only.

	N.	E.	S.	W.
Winter	17·0	18·5	30·4	24·6
Spring	20·9	23·5	21·0	26·3
Summer.................	16·5	11·7	22·8	33·2
Autumn.................	20·0	16·4	20·5	28·0

IX.—*Note on a Differential Equation.* By A. CAYLEY, Esq., M.A., F.R.S., Honorary Member of the Society.

Read February 18th, 1862.

THE following investigation was suggested to me by Mr. Harley's "Remarks on the Theory of the Transcendental Solution of Algebraic Equations," communicated to the Society at the Meeting of the 4th of February.

Mr. Harley's equation

$$y^n - ny + (n-1)\,x = 0,$$

may be written

$$y = \frac{n-1}{n}\,x + \frac{1}{n}\,y^n;$$

or putting

$$\frac{n-1}{n}\,x = u,\ \frac{1}{n} = a,$$

it becomes

$$y = u + ay^n,$$

which equation may be considered instead of the original equation; and it is to be shown that y, regarded as a function of u, satisfies a certain linear differential equation of the order $n-1$. In fact, expanding y by Lagrange's theorem, we have

$$y=u+au^n+\frac{a^2}{1.2}(u^{2n})'+\frac{a^3}{1.2.3}(u^{3n})''+\&c.$$

$$=u+au^n+\frac{a^2}{1.2}2n.u^{2n-1}+\frac{a^3}{1.2.3}3n(3n-1)u^{3n-2}+\&c.,$$

the law whereof is obvious, and using the ordinary notation of factorials, viz. $[n]^r=n\,(n-1)\ldots(n-r+1)$, we may write

$$y=\mathrm{S}_\theta\,.\,\frac{[n\theta]^{\theta-1}}{[\theta]^\theta}\,a^\theta\,u^{(n-1)\theta+1},$$

where θ extends from 0 to ∞.

It is now very easy to show that y satisfies the differential equation

$$\left[u\frac{d}{du}\right]^{n-1}y=na\left[\frac{n}{n-1}u\frac{d}{du}-\frac{2n-1}{n-1}\right]^{n-1}u^{n-1}y.$$

In fact, using on the left-hand side the foregoing value of y, and on the right-hand side the following value of $u^{n-1}y$, obtained from that of y by writing $\theta-1$ in the place of θ, viz.

$$u^{n-1}y=\mathrm{S}_\theta\,\frac{[n\theta-n]^{\theta-2}}{[\theta-1]^{\theta-1}}\,a^{\theta-1}\,u^{(n-1)\theta+1},$$

and observing that in general the symbol $u\dfrac{d}{du}$, as regards u^m, is $=m$, the equation in question will be satisfied, if only

$$\frac{[n\theta]^{\theta-1}}{[\theta]^\theta}\,[(n-1)\theta+1]^{n-1}$$

$$=\frac{n\,[n\theta-n]^{\theta-2}}{[\theta-1]^{\theta-1}}\left[\frac{n}{n-1}\big((n-1)\theta+1\big)-\frac{2n-1}{n-1}\right]^{n-1},$$

where the right-hand side is

$$=\frac{n[n\theta-n]^{\theta-2}}{[\theta-1]^{\theta-1}}[n\theta-1]^{n-1};$$

and the equation may be written

$$\frac{n\theta\,[n\theta-1]^{\theta-2}}{\theta\,[\theta-1]^{\theta-1}}[(n-1)\,\theta+1]^{n-1}=\frac{n\,[n\theta-n]^{\theta-2}}{[\theta-1]^{\theta-1}}[n\theta-1]^{n-1},$$

that is,

$$[n\theta-1]^{\theta-2}\,[(n-1)\,\theta+1]^{n-1}=[n\theta-1]^{n-1}\,[n\theta-n]^{\theta-2},$$

which, since each side of the equation is $=[n\theta-1]^{\theta+n-3}$, is obviously true.

The foregoing differential equation is developable in the form

$$\left\{\alpha_0+\alpha_1 u\frac{d}{du}+\alpha_2 u^2\left(\frac{d}{du}\right)^2\ldots+\alpha_{n-1}u^{n-1}\left(\frac{d}{du}\right)^{n-1}\right\}y$$
$$=\frac{1}{na}\left(\frac{d}{du}\right)^{n-1}y;$$

but to find the coefficients $\alpha_0, \alpha_1, \ldots \alpha_{n-1}$, I start from this form, and proceed to substitute in the equation the value of y, which on the left-hand side I use in the original form, and on the right-hand side in the form obtained by writing $\theta+1$ in the place of θ, viz.

$$y=\mathrm{S}_\theta\frac{[n(\theta+1)]^\theta}{[\theta+1]^{\theta+1}}\alpha^{\theta+1}u^{(n-1)\theta+n}.$$

The equation to be satisfied is

$$\frac{[n\theta]^{\theta-1}}{[\theta]^\theta}\Big(\alpha_0+\alpha_1[(n-1)\theta+1]^1+\alpha_2[(n-1)\theta+1]^2\ldots$$
$$+\alpha_{n-1}[(n-1)\theta+1]^{n-1}\Big)=\frac{1}{n}\frac{[n(\theta+1)]^\theta}{[\theta+1]^{\theta+1}}[(n-1)\,\theta+n]^{n-1},$$

or, what is the same thing,

$$\frac{1}{[\theta]^\theta}\Big(\alpha_0[n\theta]^{\theta-1}+\alpha_1[n\theta]^\theta+\alpha_2[n\theta]^{\theta+1}\ldots$$
$$+\alpha_{n-1}[n\theta]^{\theta+n-2}\Big)=\frac{1}{n}\frac{[n(\theta+1)]^{\theta+n-1}}{[\theta+1]^{\theta+1}}.$$

Or, observing that the right-hand side may be written

$$\frac{1}{n}\cdot\frac{n(\theta+1)\,[n\theta+n-1]^{\theta+n-2}}{(\theta+1)\,[\theta]^{\theta}},$$

the equation becomes

$$\alpha_0[n\theta]^{\theta-1}+\alpha_1[n\theta]^{\theta}+\alpha_2[n\theta]^{\theta+1}\ldots+\alpha_{n-1}[n\theta]^{\theta+n-2}$$
$$=[n\theta+n-1]^{\theta+n-2},$$

or, what is the same thing,

$$\alpha_0+\alpha_1[(n-1)\theta+1]^1+\alpha_2[(n-1)\theta+1]^2\ldots$$
$$+\alpha_{n-1}[(n-1)\theta+1]^{n-1}=[n\theta+n-1]^{n-1};$$

so that $\alpha_0, \alpha_1, \ldots \alpha_{n-1}$ are the coefficients of the expansion of $[n\theta+n-1]^{n-1}$ (which is a rational and integral function of θ, of the degree $n-1$) in a factorial series, as shown by the left-hand side of the equation.

To determine the actual values, write

$$(n-1)\theta+1=\phi,$$

this gives

$$n\theta+n-1=\frac{n\phi+n^2-3n+1}{n-1};$$

and we have therefore

$$\left[\frac{n\phi+n^2-3n+1}{n-1}\right]^{n-1}=\alpha_0+\alpha_1[\phi]^1+\alpha_2[\phi]^2\ldots$$
$$+\alpha_{n-1}[\phi]^{n-1};$$

so that the general expression is

$$\alpha_s=\frac{1}{[s]^s}\Delta^s\left(\frac{n\phi+n^2-3n+1}{n-1}\right),$$

where Δ denotes the difference in regard to ϕ ($\Delta U_\phi=U_{\phi+1}-U_\phi$), and, after the operation Δ^s is performed, ϕ is to be put equal to zero.

X.—*On some Amalgams.*

By J. P. Joule, LL.D., F.R.S. &c.

Read January 7th, 1862.

The experiments I am about to describe were made twelve years ago, but their publication was delayed to the present time in the hope of being able to extend them. Although I have not found an opportunity of doing so, I trust that these comparatively old observations will be deemed of sufficient interest to justify me in having submitted them to the Society.

My attention was first directed to the subject through my wish to discover a ready means of procuring a perfectly true and polished metallic surface. Since it was believed that mercury refused to enter into combination with iron, I thought that by depositing the latter on mercury, a plate of it would be formed possessing a smoothness equal to that of the fluid metal. However, on making the experiment, I found that the iron entered into combination with the mercury, forming an amalgam *.

One element of a Daniell's battery was amply sufficient for the purpose. Its zinc plate was connected by a wire with a globule of mercury covered by a solution of sulphate of iron, whilst an iron wire attached to its copper plate, and dipping into the solution, completed the circuit. The iron wire gradually dissolved, whilst an equal portion was taken up by the mercury, which, in doing so, by de-

* In consequence of iron possessing nearly the same affinity as hydrogen for oxygen, there is considerable difficulty in depositing it electro-chemically on a metallic plate. I have only once or twice obtained a good electrotype deposit on a polished surface, to which the iron adhered so firmly that it could only be removed by abrasion. Even in the process of amalgamating iron, the constant evolution of hydrogen from the mercury shows that decomposition of water takes place simultaneously with that of the salt of iron.

grees lost its fluidity, until at length a mass of crystals of amalgam was formed having a greyish-white colour of metallic brilliancy. The time required to complete the operation was generally about one day; but a longer or shorter period was occupied in some instances, in consequence of variations in the quantity of mercury employed, and in the efficiency of the voltaic arrangement. The following Table contains the results of most of the experiments made on the amalgam of iron. The analysis of this and other amalgams was made by heating them in a glass tube through which a current of hydrogen was passed.

No.	Composition.		Sp. grav.	Remarks.
	Mercury.	Iron.		
1.	100	0·143	....	Perfectly fluid.
2.	,,	1·39	....	Fluid.
3.	,,	2·97	....	Semifluid.
4.	,,	11·8	12·19	Soft.
5.	,,	18·3	....	Solid: colour, greyish white.
6.	,,	47·5	....	Solid: good metallic lustre.
7.	,,	127·6	10·11	Solid: friable.
8.	,,	14·74	....	The superfluous mercury pressed out from the semifluid amalgam by hand.
9.	,,	79	....	Compressed rapidly, and with a force of fifty tons on the square inch.
10.	,,	103·2	....	Ditto.

No. 5 of the above Table was a solid amalgam of a greyish white, approaching the colour of iron. It could be easily broken into powder. When dried and left undisturbed, it soon became covered with small globules of mercury, until ultimately it was entirely decomposed.

To obtain No. 6, I used a solution of chloride of iron instead of the sulphate which was used in all the other experiments.

No. 7 could be easily reduced to powder. It had a bluish colour, and was destitute of metallic lustre until it was rubbed. It remained some time under water without change, but when dried became speedily decomposed,

whether it was exposed to the action of air, or was placed under the exhausted receiver of an air-pump.

The amalgam of iron, whether solid or fluid, is attracted by the magnet, and in the solid condition is capable of receiving a slight dose of permanent magnetization.

In No. 1, the iron, though apparently completely dissolved by the mercury, remained in the full possession of its magnetic virtue.

A portion of No. 2, weighing 87·69 grains, placed in a piece of quill, was attracted by a magnet with a force equal to 0·36 gr. 3·058 grains of iron wire, cut into small pieces and placed in the same quill, were attracted by a force of 0·94 gr. The quantity of iron contained by the amalgam was 1·2 grain. Hence it appeared that the iron had lost very little of its magnetic virtue by combination with the mercury.

The following observations were made to discover the position of the amalgam of iron in the electro-chemical series. The galvanometer which was employed had a coil 1 foot in diameter, composed of 400 convolutions of wire 1-40th of an inch in diameter.

Positive Metal.	Negative Metal.	Deflection.
Amalgamated zinc.	Zinc.	10°
Zinc.	Iron.	42°
Zinc.	Copper.	65°
Amalgamated iron.	Copper.	15°
Iron.	Amalgamated iron.	5°

It appears, therefore, that the amalgamation of iron produces a contrary effect to the amalgamation of zinc. This is especially remarkable, as the amalgamated iron contained no carbon, which must have existed to a certain extent in the plate of iron with which it was associated.

When amalgam of iron is left under water for a few days, it becomes coated with rust. If shaken violently, it

becomes almost immediately decomposed, the iron as a black powder floating on the surface of the liberated mercury.

When the amalgam is heated to the boiling-point of mercury, the liberated iron unites with the oxygen of the air, throwing off bright red sparks, and leaving a hard lump of oxide.

The experiments seem to indicate that the solid amalgam of iron which contains the largest quantity of mercury is a binary combination of the two metals.

The specimen marked No. 8 in the Table was procured by compressing by hand between folds of linen a quantity of amalgam in a soft state. There resulted a mass of white crystals of perfect metallic lustre. The mercury left was about two equivalents. It seemed probable that one of these was left uncombined among the pores of the amalgam.

The specimens Nos. 9 and 10 were obtained by hydraulic pressure acting on a piston of steel 3-8ths of an inch in diameter, working in a cylinder into which a silken bag filled with amalgam was placed. The resulting amalgam was so hard, that it could only be broken by the smart blow of a hammer. Its black colour seemed to indicate nearly total decomposition.

Amalgam of Copper.—To form this amalgam, a small quantity of mercury was poured into a dish containing solution of sulphate of copper. A copper wire connected the mercury with the zinc of a Daniell's cell, whilst a coil of copper wire immersed in the cupreous solution completed the circuit. A mass of crystals was gradually formed, branching out to the distance of half an inch or more. Ultimately pure copper was deposited on the extremities of the crystals in a fringe of light red, the whole presenting the appearance of a beautiful flower. In the following Table I have collected the results of several such experiments:—

No.	Mercury.	Copper.	Sp. grav.	Remarks.
1.	100	22·5	13·32	Arborescent crystals: no pink deposit.
2.	"	24·73	13·260	Ditto ditto.
3.	"	25	13·185	Ditto ditto.
4.	"	27·76	13·17	Ditto ditto.
5.	"	29·92	....	Pink deposit on the extremities of the crystals.
6.	"	37·7	....	Pink deposit over the greatest part.
7.	"	31·35	13·51	The copper which was deposited on the outside of the crystals was constantly removed. The experiment was stopped when the central button of amalgam became pink in one or two places.
8.	"	29·08	....	Ditto ditto.
9.	"	29·0	13·76	Ditto ditto.
10.	"	34·19	13·01	From a hot solution of sulphate of copper. Hard and crystalline mass.
11.	"	39·64	12·99	Formed slowly in eight days. Pink in several places.
12.	"	41·5	....	This amalgam was continually pounded whilst it was being produced. Pink in several places.
13.	"	38·12	12·65	Sulphate of copper, kept at 100° Fahr. In two days the amalgam was covered with arborescent crystals tipped with pink.

On inspecting the above Table, it will appear that whenever the quantity of copper approaches nearly to an equivalent, a deposit of unamalgamated copper begins to take place. This seems to demonstrate that the solid amalgam containing the least quantity of mercury is a binary compound.

The mean of the specific gravities of the specimens possessing an equivalent (or a little less than an equivalent) of copper is 13·31.

The specific gravity of the other amalgams, containing excess of copper, is 12·82. It follows that, if we admit that the specific gravity of copper is not altered when it enters into combination with mercury, the specific gravity of the latter, in the amalgam, is 15·415.

In the following Table I give the analysis of amalgams after pressure of various degrees of force had been applied during various lengths of time:—

No.	Pressure per square inch, in tons.	Time.	Mercury.	Copper.	Sp. grav.
1.	$\frac{3}{4}$	12 hours	100	20·3	
2.	$\frac{3}{4}$	12 hours	,,	17·28	
3.	1	36 hours	,,	20·5	
4.	$1\frac{1}{4}$	17 hours	,,	18·95	
5.	2	12 hours	,,	18·4	
6.	gradually increased up to 1 ton	$3\frac{1}{2}$ months	,,	39·02	12·76
7.	,,	13 days, with intervals amounting to 54 days	,,	38·43	12·56
8.	9	a few minutes	,,	25·84	12·92
9.	15	,,	,,	28·57	
10.	18	,,	,,	28·4	13·01
11.	20	,,	,,	29·46	
12.	72	,,	,,	30·95	13·06
13.	72	,,	,,	32·82	12·93
14.	144	,,	,,	35·13	12·96
15.	144	,,	,,	34·87	12·57
16.	144	,,	,,	35·63	12·62
17.	20	30 minutes	,,	33·04	
18.	36	30 minutes	,,	30·25	12·88
19.	72	1 hour	,,	32·34	
20.	30	2 hours	,,	40·18	
21.	30	7 hours	,,	44·34	12·38

On inspecting the above Table, it will appear that a moderate pressure continued for a short time leaves a binary compound of the metals along with the quantity of mercury which may be supposed to be entangled among the crystals. When the pressure was very great, or was continued for a long time, the resulting amalgam invariably contained more than one equivalent of copper. I believe that this arises from a decomposition of the binary amalgam by the violent mechanical means adopted.

On the supposition that the copper retains its own specific gravity, the density of the above amalgams gives for the mercury a specific gravity of 14·985.

The *Amalgam of Silver* was generally produced by treating mercury with nitrate of silver. The action goes on until a hard mass of shining crystals is formed, consisting of about an equivalent of silver to one of mercury.

No.	Mercury.	Silver.	Sp. grav.	Remarks.
1.	100	52·6	14·68	From cold solution of nitrate of silver.
2.	,,	100·3	....	Ditto ditto.
3.	,,	115·4	....	Ditto ditto.
4.	,,	115·2	13·25	Ditto ditto.
5.	,,	155·8	12·34	Boiled in solution of nitrate of silver.
6.	,,	106·4	12·49	From a hot concentrated solution of nitrate of silver.
7.	,,	293·3	12·54	Button of amalgam formed by the electrolytic action of one cell of Daniell's battery.
8.	,,	2614·0	11·42	Crystals formed on the edges of the above button of amalgam.

From the above Table, it appears that the amalgam most readily formed by the action of nitrate of silver on mercury is a binary compound, for the average result gives the proportion of 107·6 silver to 100 mercury. It will be noticed that the specific gravity of the specimens indicates, as in the case of the amalgam of copper, a very considerable contraction of volume, principally referable no doubt to the assumption of the solid state by the mercury, the specific gravity of which comes out 16·5 from the above and succeeding experiments on the amalgam of silver.

In the next Table I give the composition of amalgams of silver after compression. Before placing it in the press, each specimen was mixed up with excess of mercury so as to form a thick paste. I should mention here, that, on making the analysis, it was found necessary to employ a temperature nearly sufficient to fuse the silver in order to drive from it the last portions of mercury.

No.	Pressure.	Mercury.	Silver.	Sp. grav.
1.	$2\frac{1}{2}$ tons for 1 day	100	33·78	
2.	3 tons for 3 days	,,	37·76	
3.	72 tons for 1 hour	,,	40·13	13·61
4.	72 tons for $1\frac{1}{2}$ hour	,,	40	13·78
5.	72 tons for $1\frac{1}{2}$ hour	,,	51·55	13·44
6.	72 tons for 20′	,,	43·15	

The mean composition of the amalgam, after being pressed with 72 tons on the inch, was therefore 43·71 silver to 100 mercury. Allowing for mercury remaining among the crystals in an uncombined state, we may conclude that the solid amalgam containing the largest quantity of mercury is composed of one equivalent of silver to two of mercury.

Amalgam of Platinum.—To obtain this amalgam, platinum was deposited on mercury by the electrolytic action of two or three voltaic cells on the bichloride.

No.	Mercury.	Platinum.	Sp. grav.	Remarks.
1.	100	15·48	14·29	Metallic lustre when rubbed.
2.	,,	21·6	....	Solid. Dark grey colour.
3.	,,	34·76	14·60	Dark grey; no metallic lustre.

An amalgam of 12 platinum to 100 mercury possesses a bright metallic lustre, and is soft and greasy to the touch. Pressed with a force of 72 tons to the square inch, a hard button of dark grey amalgam is left, consisting of 43·2 parts of platinum to 100 of mercury. I infer therefore that the solid amalgam of platinum, which contains the largest quantity of mercury, is composed of two equivalents of mercury to one of platinum *.

The specific gravity of this amalgam appears to be nearly that which it would be on the supposition that no condensation of volume takes place on the union of the metals; but the specimens were too small to make very accurate determinations of specific gravity.

Amalgam of Zinc was obtained electrolytically from sulphate of zinc; after some time the mercury lost its fluidity, and branching crystals began to be formed.

* Amalgam of platinum in the form of a thick paste may be obtained by exposing mercury to the action of bichloride of platinum for a sufficient length of time.

No.	Mercury.	Zinc.	Sp. grav.	Remarks.
1.	100	39·4	11·34	White and crystalline.
2.	,,	122·8	8·935	Ditto ditto.
3.	,,	184·9	8·349	Prepared from hot sulphate of zinc.

The first of the above three specimens, consisting of an equivalent of each metal, appears to be the amalgam which, containing the largest quantity of mercury, is yet solid. The specific gravity indicates a certain contraction of volume, but not nearly as much as that in the amalgams of silver and copper, but such as would place the specific gravity of the mercury at 14·1. Pressure seemed to have the effect of decomposing this amalgam, or at least of expelling mercury, until the amalgam consisted of about one equivalent of mercury to three of zinc.

No.	Pressure.	Mercury.	Zinc.
1.	$\frac{3}{4}$ ton for 1 day	100	59·25
2.	$1\frac{1}{4}$ ton for 1 day	,,	69
3.	50 tons for 1 hour	,,	76·7
4.	ditto	,,	79·6
5.	ditto	,,	75·9

Amalgam of Lead.—On making mercury negative in acetate of lead, a crystalline amalgam was gradually formed. The operation was stopped when the characteristic flat blue crystals of lead began to make their appearance. The amalgam was found to have a specific gravity of 12·64 (indicating 13·85 for its mercury), and to consist of 100 mercury to 69·83 lead, and, allowing for unavoidable excess of mercury, may be considered as a binary compound.

To ascertain the effect of pressure, a liquid amalgam was formed by heating the two metals together. It was then compressed with a force of three tons to the square inch for a day. A greater pressure than this would have caused the amalgam as well as the mercury to escape from the press. The result was a mass of bright crystals, easily fractured, which had a specific gravity of 12·11, and was

composed of 100 mercury to 194 lead. I think there can be no doubt that the pressure had partly decomposed the binary compound. It appears that little or no contraction of volume is occasioned by the combination of the metals.

Amalgam of Tin was obtained by making mercury negative in a solution of chloride of tin.

No.	Mercury.	Tin.	Sp. grav.	Remarks.
1.	100	51·01	10·518	Beautiful crystalline amalgam.
2.	„	44·12	10·94	Ditto ditto.
3.	„	70·7	....	Some unamalgamated tin crystals at the extremities of the amalgam.

The amalgam formed by the electrolytic process appears, therefore, to be a binary compound. Its specific gravity, along with that given in the next Table, shows a specific gravity of 14·1 for the mercury in combination. Pressure of the amalgam gave the following results :—

No.	Pressure.	Mercury.	Tin.	Sp. gr.	Remarks.
1.	1440 lbs. for 10′	100	75·9		
2.	1440 lbs. for 2 days	„	255·5		
3.	2724 lbs. for 2 days	„	392·4		
4.	5400 lbs. during 30 days	„	384·1	8·154	Pressure gradually increased.
5.	50 tons for 15′	„	402·3		
6.	2700 lbs. during 30 days	„	408·9	......	Pressure of 50 tons during 1 day did not afterwards drive out more mercury.

The above results show most decisively that pressure is able to decompose the amalgam of tin, the mercury left after long-continued high pressure having a volume little more than one-eighth of the entire mass.

I made an unsuccessful attempt to amalgamate hydrogen, by developing it at a low temperature (4° Fahr.) on mercury. It did not appear that the smallest quantity of hydrogen was taken up. This appears, however, to be an

experiment worth repeating. I think it highly probable that, by using intense cold and very great pressure, an amalgam of hydrogen might be formed.

As metals generally retain their specific gravities when they meet to form alloys, it may be inferred that the foregoing experiments indicate the specific gravity of mercury in the solid state. This value, from the average of the thirty-six determinations of specific gravity above given, is 15·19.

XI.—*On the Convective Equilibrium of Temperature in the Atmosphere.* By Prof. Wm. Thomson, M.A., LL.D., F.R.S., &c.

Read January 21st, 1862.

The particles composing any fluid mass are subject to various changing influences, in particular of pressure, whenever they are moved from one situation to another. In this way they experience changes of temperature altogether independent of the effects produced by the radiation or conduction of heat. When all the parts of a fluid are freely interchanged and not sensibly influenced by radiation and conduction, the temperature of the fluid is said to be in a state of convective equilibrium. The equations of convective equilibrium in the atmosphere are as follows, Π, T, and W denoting the pressure, temperature, and mass per cubic foot of the air at the earth's surface, and p, t, and ρ the same qualities of the air at any height x:—

$$\left(\frac{p}{\Pi}\right)^{1-\frac{1}{k}}=\frac{t}{T}*, \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

which is the known relation between temperature and pressure;

* For proof, see foot-note, p. 129, below.

$$\frac{p}{\Pi}=\left(\frac{\rho}{W}\right)^k, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

the deduced relation between pressure and density; and

$$dp=-\rho\, dx, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

the hydrostatic equation, the variation of gravity at different heights being neglected, and the weight of unit mass (1 lb.) being taken as unit of force. Hence by integration,

$\frac{t}{T}=1-\frac{Wx}{\Pi}\cdot\frac{k-1}{k}$, Or if, for brevity, we denote $\frac{\Pi}{W}$ by H,

$$\frac{t}{T}=1-\frac{x}{H}\cdot\frac{k-1}{k}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

From (4), (1), and (2), it appears that temperature, pressure, and density would all vanish at the very moderate height $\frac{1{\cdot}41}{{\cdot}41}\times H$, which is about 90100 feet, or between 17 and 18 miles, if convective equilibrium existed and if the gaseous laws had application to so low temperatures and densities. It has always appeared to me to be most improbable that there is any limit to our atmosphere; and no one can suppose that there is a limit at any height nearly so small as 17 or 18 miles. It is difficult to make even a plausible conjecture as to the effects of deviations from the gaseous laws in circumstances of which we know so little as those of air at very low temperatures; but it seems certain that the other hypothesis involved in the preceding equations is violated by actions tending to *heat* the air in the higher regions. For at moderate elevations above the surface, where we have air following very strictly the gaseous laws, the rate of decrease of temperature would, according to equation (4), be $\frac{{\cdot}41\times T}{1{\cdot}41\, H}$ per foot, that is to say, $\frac{1^\circ}{329}$ per foot, since $H=26224\times\frac{T}{274}$ or 1° Cent. per 329 feet. Now, the actual decrease, according to Mr.

Welsh, is 1° Cent. in 530 feet, or not much more than half that according to convective equilibrium.

It seems that radiation, instead of partially accounting for the greater warmth of the air below, as commonly supposed, may actually diminish the cooling effect, in going up, which convection produces. In fact, since direct conduction is certainly insensible, we have only convection and radiation to deal with, except when condensations of moisture, &c., have to be taken into account. In fair and cloudless weather, then, the lower and lowest air being on the whole warmer (the lowest being of course at the same temperature as the earth's surface), it is perfectly certain that the upper air must gain heat by radiation from the lower—and that the convective difference of temperature must be diminished by the mutual interradiation.

There are difficulties connected with the radiation of air and earth out into space, and of heat from the sun to air and earth; but I think a full consideration of all the circumstances must explain the smallness of the decrease of temperature which observation shows.

Dr. Joule having suggested that condensation of vapour in upward currents of air might account, to a considerable extent if not perfectly, for the smallness of the lowering of temperature actually found in going up, I have added the following investigation, in which the effect of condensation is taken into account.

If a quantity of air, dry or moist, is allowed to expand from bulk v to bulk $v+dv$, it will do an amount of work equal to pdv on the surrounding matter. Now, by the principle established approximately by Dr. Joule, in his experiments on air in 1844 *, the change of temperature which the mass will experience will be almost exactly

* "On the Changes of Temperature produced by the Rarefaction and Condensation of Air," communicated to the Royal Society, June 20, 1844, and published in the 'Philosophical Magazine,' 1845, first half year.

equal to what would be produced by keeping it at constant volume, $v+dv$, and removing a quantity of heat equal to the thermal equivalent of pdv. This is expressed by $\frac{1}{J}pdv$, if we adopt the usual notation, J, for the dynamical equivalent of the thermal unit. Now, if t and $t+dt$ denote the primitive and the cooled temperatures, so that $-dt$ expresses the cooling effect (which is positive, dt being negative), the bulk of the vapour, if at saturation in each case, would tend to be $v\frac{s+ds}{s}$ if s denote the volume of a pound of vapour at saturation at any temperature t, and $s+ds$ its volume at temperature $t+dt$. Hence if, as it will be seen is the case, $v\frac{ds}{s}$ is greater than dv, a portion equal in bulk to $v\frac{ds}{s}-dv$ of the water primitively in vapour, must become condensed. Hence the abstraction of the heat $\frac{1}{J}pdv$ produces two effects; it cools the mass of air at constant volume from temperature t to temperature $t+dt$, and it condenses a bulk

$$v\frac{ds}{s}-dv$$

of vapour. Hence, if L denote the latent heat of a cubic foot of vapour of water at temperature t, and N the specific heat of one pound of air in constant volume, we have

$$\frac{1}{J}pdv=N\times(-dt)+L\left(v\frac{ds}{s}-dv,\right)^*$$

if we suppose the mass of air considered to weigh 1 lb.

* If $L=0$, this equation becomes

$$\frac{1}{J}pdv=N\times(-dt),$$

(with or without the vapour, which will make but little difference on the whole weight). Hence

$$\frac{dv}{-dt}=\frac{\mathrm{JN}+\mathrm{JL}v\dfrac{d\log s}{-dt}}{p+\mathrm{JL}},$$

where, for brevity, $d\log s$ is written in place of $\frac{ds}{s}$, $\log s$ denoting the Napierian logarithm of s.

To find L and $\frac{d\log s}{-dt}$, it is necessary to know the bulk of a pound of steam at different temperatures. Dr. Joule and I have demonstrated*, by experiments on air and by dynamical reasoning, that

$$\mathrm{L}=\frac{\mathrm{J}}{t}\frac{dp}{dt}\left(1-\frac{\lambda}{\gamma}\right),$$

where p denotes the pressure of vapour at saturation at the temperature t, and $\frac{\lambda}{\gamma}$ denotes the ratio of the bulk of liquid to vapour. Since $\frac{\lambda}{\gamma}$ is very small, we have $\mathrm{L}=\frac{\mathrm{J}}{t}\frac{dp}{dt}$ approximately.

It was shown also in the same Paper, that the density of saturated vapour was to be obtained more accurately from this equation, and Regnault's experiments on the latent

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or, since $\mathrm{JN}=\frac{pv}{t}\cdot\frac{1}{k-1}$ (by an elementary thermodynamic formula for a perfect gas),

$$\frac{dv}{v}=\frac{1}{k-1}\times\frac{-dt}{t},$$

whence, by integration, $\frac{t}{\mathrm{T}}=\left(\frac{\mathrm{V}}{v}\right)^{k-1}$.

This expresses the elevation of temperature experienced by a perfect gas when compressed and not allowed to part with heat.

* On the Thermal Effects of Fluids in Motion, Part II., Theoretical Deductions, Section II., Transactions of the Royal Society, June 1854.

heat of a stated weight of vapour, than from any direct experiments on the density of vapour made up to that time. This conclusion has been verified by the recent experiments of Messrs. Fairbairn and Tate. With the assistance of some excellent tables in Rankine's "Steam Engine and other Prime Movers," calculated on these principles, I have obtained the following results:—

Temperature Centigrade, or $t-273\cdot7$.	Volume of 1 lb. of air at pressure 2117 lbs. per square foot.	Dynamical value of latent heat of 1 cubic foot of saturated vapour.	Proportionate diminution of bulk of saturated vapour per 1° Cent. of elevation of temperature.	Augmentation of volume of 1 lb. of moist air required to cool it 1° Cent.	Elevation from earth's surface required to cool moist air by 1° Cent.
$t-273\cdot7$	v	J L	$\frac{d \log s}{-dt}$	$\frac{dv}{-dt}$	$\frac{dx}{-dt}$
°	cubic ft.	ft. lbs.		cubic ft.	feet.
0	12·38	249	·0698	·1905	499
5	12·61	348	·0671	·2150	551
10	12·83	481	·0644	·2434	611
15	13·06	655	·0617	·2753	678
20	13·29	881	·0592	·3096	751
25	13·52	1171	·0569	·3455	827
30	13·74	1538	·0546	·3800	900
35	13·97	1999	·0524	·3950	932

The column of this Table headed $\frac{dv}{-dt}$ is calculated from the preceding formula. It expresses the expansion on the bulk of a cubic foot required to produce a cooling effect $-dt$ (along with an infinitesimal lowering of pressure below the standard pressure of 2117 lbs. per square foot, denoted by p), when the mass is not allowed either to absorb or to emit heat.

The last column $\left(\text{headed } \frac{dx}{-dt}\right)$ is calculated from the column headed $\frac{dv}{-dt}$ by the following formula,

$$dx = pdv + pv\frac{-dt}{t},$$

and shows the height, dx, that must be reached to get a lowering of temperature, $-dt$, when air saturated with moisture ascends. The pressure, p, is taken as 2117 lbs. per square foot; and the value of $\frac{v}{t}$, which is the same for the same pressure, whatever is the temperature, is $\frac{12 \cdot 38}{274}$. The results, for temperatures from 0° to 35° Cent., are exhibited in the last column of the Table. For the temperatures 0°, 5°, and 10°, they agree very well with the height in which Mr. Welsh found a lowering of temperature of 1° Cent.; and we may conclude that at the times and places of his observations the lowering of temperature upwards was nearly the same as that which air saturated with moisture would experience in ascending.

It is to be remarked that, except when the air is saturated, and when, therefore, an ascending current will always keep forming cloud, the effect of vapour of water, however near saturation, will be scarcely sensible on the cooling effect of expansion. Hence the law of convective equilibrium of temperature in upward or downward currents of cloudless air must agree very closely with that investigated above, and must give a variation of 1° Cent. in not much more or less than 330 feet.

It appears, therefore, that the explanation suggested by Dr. Joule is correct; and that the condensation of vapour in ascending air is the chief cause of the cooling effect being so much less than that which would be experienced by dry air.

XII.—*On the Relations between the Decrement of Temperature on ascending in the Atmosphere, and other Meteorological Elements.* By Joseph Baxendell, Esq., F.R.A.S.

Read before the Physical and Mathematical Section, February 27th, 1862.

According to the theory which attributes the production of winds and storms to upward currents of air caused by the heat liberated during the condensation of aqueous vapour into clouds and rain, the rate of decrease of temperature on ascending in the atmosphere ought to be less in rainy than in fair weather; and the reasonings and calculations of Dr. Wm. Thomson, in his valuable paper "On the Convective Equilibrium of Temperature in the Atmosphere," lately read to the Society, point to the same conclusion; but in a paper entitled "Remarks on the Theory of Rain," read to this Section on the 29th of March, 1860, I gave some results derived from a discussion of the Greenwich and Oxford observations, which seemed to militate against this theory; and reference was made to the fact, stated by Kaemtz and others, that the diminution of temperature on ascending in the atmosphere is more rapid in rainy than in fine weather. It appears, however, that this fact is not generally admitted by meteorologists, as the observations from which it is derived were mostly of a desultory nature, and continued for only short periods of time. I have therefore thought that a discussion of the monthly results of the observations made during the years 1848–1858 at Geneva and on the Great St. Bernard, given by Mr. Vernon in his valuable paper "On the irregular Barometric Oscillations" at those places, might throw some light on the subject, and, at the same time, serve to indicate the relations which exist between the decrement of tem-

perature and other meteorological elements—a branch of meteorology which has hitherto been almost entirely neglected, although it seems likely to yield results of considerable importance to the future progress of the science.

In the Tables which accompany this paper, the data for mean monthly temperature, rainfall, and amount of barometric oscillation are taken from Mr. Vernon's paper; but the corresponding mean monthly heights of the barometer are from the Milan observations, as I had not access to the barometric observations made at Geneva and the Great St. Bernard; this will, however, not affect the general conclusions, as the variations of the barometer at Milan are generally almost precisely similar to those at Geneva.

Table I. contains the mean monthly and annual results of the comparisons of the rainfall at Geneva and the Great St. Bernard, with the differences of temperature of the two stations. Taking the mean values for the year, we find that an annual rainfall at Geneva of 19·581 in. gives a difference of temperature between the two stations of 19°·47; whilst a rainfall of 52·972 in. gives a difference of 19°·89, or 0°·4 greater; and that a rainfall of 26·745 in. on the Great St. Bernard gives a difference of 19°·38, and a fall of 69·838 in. gives a difference of 20°·09, or 0°·71 more. It is evident, therefore, that an increase in the amount of rain, either at Geneva or on the Great St. Bernard, is, on the average of the year, accompanied by an increase of the difference of temperature between the two stations.

Table II. contains all the monthly differences of temperature which are *below* the mean value, and the corresponding falls of rain, mean temperatures, and amounts of barometric oscillation, at both stations, and the mean heights of the barometer at Milan.

Table III. contains all the monthly differences of tem-

perature which are *above* the mean value, and the corresponding data for the other elements.

Tables IV. and V. contain the mean results of Tables II. and III. The final mean values for the year are as follows:—

Difference of temperature between the two Stations.	Rainfall.		Mean Temperature.		Amount of Oscillation.		Mean height of Barometer at Milan.
	Geneva.	Gt. St. Bernard.	Geneva.	Gt. St. Bernard.	Geneva.	Gt. St Bernard.	
°	in.	in.	°	°	in.	in.	in.
18·25	31·836	43·862	47·98	29·72	36·214	29·701	29·514
21·18	32·898	48·485	47·75	26·57	42·284	33·892	29·406
2·93	1·062	4·623	−0·23	−3·15	6·070	4·191	−0·108

It appears, therefore, that with a difference of temperature of 18°·25 between the lower and the higher stations the rainfall at Geneva is 31·836 in., and at the Great St. Bernard 43·862 in.; while with a difference of temperature 2°·93 greater, or 21°·18, the rainfall at Geneva is 32·898 in., and on the Great St. Bernard 48·485 in.,—the difference at Geneva being + 1·062 in., and on the Great St. Bernard + 4·623 in. The conclusion drawn from Table I. is therefore confirmed by these results,—a greater difference of temperature between the two stations being accompanied by a greater fall of rain.

With the lower difference of temperature, the mean temperature at Geneva is 47°·98, and on the Great St. Bernard 29°·72; and with the higher difference the corresponding mean temperatures are 47°·75 and 26°·57, the difference at Geneva being −0°·23, and on the Great St. Bernard −3°·15. We are therefore led to the remarkable conclusion that an increased difference of temperature is due to a diminution of temperature at the higher station, and not to an increase of temperature at the lower station, —an effect precisely the opposite of that produced by

change of season, in which a greater difference of temperature is mainly due to an increase of temperature at the lower station.

The amount of barometric oscillation at Geneva, with a difference of temperature of 18°·25, is 36·214 in., and on the Great St. Bernard 29·701 in.; and with a difference of 21°·18, the amounts are 42·284 in. and 33·892 in. respectively. The difference of the amounts at Geneva is 6·070 in., and on the Great St. Bernard 4·191 in. These results are also very remarkable, as it would naturally be supposed that an increased disturbance of the atmosphere would tend to produce a more equable distribution of temperature; but the occurrence of a barometrical pressure below the mean, with a difference of temperature and amount of oscillation both above the mean, the temperature of the lower station being also slightly lower, is still more remarkable, as it is apparently inconsistent with all the theories which have yet been advanced to account for the irregular oscillations of the barometer.

The increase in the amount of barometric oscillation, with increase of difference of temperature between the two stations, appears to indicate that the disturbances of the atmosphere which produce disturbances of barometrical pressure take place chiefly in a horizontal, and not in a vertical direction.

The diminution of mean temperature with increase of difference of temperature, and increased rainfall, points clearly to the operation of a cooling agency sufficiently powerful to neutralize completely the effects which, according to the theory above alluded to, ought to be produced by the latent heat of aqueous vapour when rendered sensible by the condensation of the vapour into clouds and rains. In my paper "On the Theory of Rain," I was led to conclude "that the formation of rain might

be regarded as a cooling process," and I remarked "that air nearly saturated with vapour had probably a greater power of radiating heat than dry air," a view which has since been abundantly confirmed by the experiments of Prof. Tyndall; but it may, nevertheless, be doubted whether radiation alone will account for the *whole* of the cooling effect indicated by the results of this inquiry; and I may therefore remark that some results of previous investigations of meteorological phenomena had led me to regard it as very probable that a portion of the heat taken up into the atmosphere by aqueous vapour is afterwards expended in the production of atmospherical electricity, and this probability seems to me to be considerably increased by the results now obtained from the Geneva and Great St. Bernard observations.

From the relations established by this investigation, it may also be concluded that in a mass of air moving from a higher to a lower latitude and acquiring an increase of temperature, the change of temperature is more rapid in the lower than in the higher strata, while, on the contrary, in a mass moving from a low to a high latitude, and losing heat, the change is most rapid in the upper strata. It also seems probable that one of the essential conditions in the formation of a rotatory or cyclonic storm is a greater difference of temperature than usual between the successive strata of the atmosphere at the point where the storm originates.

TABLE I.

	Geneva.				Great St. Bernard.			
	Rainfall below the Mean.		Rainfall above the Mean.		Rainfall below the Mean.		Rainfall above the Mean.	
	Amount of Rain.	Difference of Temperature.	Amount of Rain.	Difference of Temperature.	Amount of Rain.	Difference of Temperature.	Amount of Rain.	Difference of Temperature.
	in.	°	in.	°	in.	°	in.	°
January	1.129	16.44	3.161	17.65	2.553	16.38	6.084	17.48
February	0.830	18.27	3.524	17.85	1.918	16.87	6.719	20.53
March	0.628	19.41	2.616	21.47	1.188	18.91	5.244	21.66
April	1.670	22.03	4.887	21.54	3.588	21.94	8.844	21.83
May	2.359	22.00	7.374	22.24	3.097	22.29	7.259	21.80
June	1.974	22.69	5.265	21.85	2.363	22.58	6.649	22.14
July	1.768	22.13	4.237	21.96	1.706	22.30	4.851	21.76
August	1.781	20.80	4.356	21.00	2.000	20.94	4.005	20.88
September	2.297	20.17	4.905	21.12	1.788	20.27	3.840	21.00
October	3.009	18.76	7.598	18.53	2.855	18.22	7.059	19.22
November	1.480	18.12	3.338	18.10	2.251	18.20	4.868	18.00
December	0.656	12.82	1.711	15.48	1.438	13.70	4.416	14.83
	19.581	19.47	52.972	19.89	26.745	19.38	69.838	20.09

Table II.

Year.	Difference of Mean Temperatures of the two Stations.	Geneva. Rainfall.	Geneva. Mean Temperature.	Geneva. Amount of Barometric Oscillation.	Milan. Mean height of Barometer.	Great St. Bernard. Rainfall.	Great St. Bernard. Mean Temperature.	Great St. Bernard. Amount of Oscillation.
JANUARY.								
	°	in.	°	in.	in.	in.	°	in.
1848.	16·1	0·858	24·5	3·641	29·408	3·398	8·4	
1850.	15·3	1·634	27·5	5·077	·469	5·773	12·2	4·101
1851.	15·2	2·020	33·3	3·640	·614	3·819	18·1	2·729
1852.	16·7	1·803	36·1	4·224	·657	4·244	19·4	3·454
1854.	13·8	0·843	31·8	4·035	·520	3·854	18·0	3·216
1855.	16·1	1·354	29·2	3·316	·535	2·563	13·1	3·138
1858.	15·5	0·181	27·5	2·961	·766	0·641	12·0	2·770
	15·53	1·242	29·98	3·842	29·567	3·470	14·45	3·234
FEBRUARY.								
1849.	14·8	1·120	36·5	2·947	29·727	2·441	21·7	2·560
1850.	17·3	0·925	39·2	3·984	·603	5·189	21·9	3·758
1851.	17·9	1·031	34·3	3·152	·547	3·350	16·4	2·389
1855.	17·3	5·614	35·2	4·595	·236	3·531	17·9	3·073
1856.	14·7	1·094	37·8	3·386	·583	1·701	23·1	2·318
1857.	15·1	0·705	31·6	1·519	·741	0·000	16·5	1·280
	16·18	1·748	35·76	3·264	29·572	2·702	19·58	2·561
MARCH.								
1849.	19·6	1·088	37·8	5·026	29·470	4·004	18·2	4·350
1850.	19·1	0·185	36·3	4·008	·553	1·516	17·2	3·337
1852.	18·4	0·299	36·7	3·865	·526	1·854	18·3	3·573
1854.	18·7	0·055	40·1	2·218	·725	0·303	21·4	1·845
1856.	18·2	2·457	40·4	3·011	·585	0·657	22·2	2·297
1857.	18·4	1·040	39·4	3·132	·395	0·886	21·0	2·888
	18·73	0·854	38·45	3·543	29·542	1·536	19·71	3·048
APRIL.								
1848.	20·3	6·012	49·2	3·904	29·394	11·008	28·9	2·436
1850.	21·2	5·070	46·5	3·931	·337	5·249	25·3	2·360
1851.	21·5	2·280	48·4	3·142	·385	8·756	26·9	2·397
1852.	21·4	0·378	46·3	2·794	·427	3·248	24·9	2·116
1855.	21·5	0·815	46·2	3·295	·379	3·390	24·7	3·138
	21·18	2·911	47·32	3·415	29·384	6·330	26·14	2·489

TABLE II. (*continued*).

Year.	Difference of Mean Temperatures of the two Stations.	Geneva. Rainfall.	Geneva. Mean Temperature.	Geneva. Amount of Barometric Oscillation.	Milan. Mean height of Barometer.	Great St. Bernard. Rainfall.	Great St. Bernard. Mean Temperature.	Great St. Bernard. Amount of Oscillation.
MAY.								
	°	in.	°	in.	in.	in.	°	in.
1848.	19·8	0·772	56·7	2·372	29·489	0·240	36·9	1·713
1849.	20·7	3·177	55·2	2·722	·453	6·827	34·5	1·668
1851.	21·0	1·949	50·0	2·587	·428	6·342	29·0	2·111
1853.	21·6	5·953	52·5	3·298	·366	6·468	30·9	2·919
1857.	21·9	2·157	54·9	2·341	·369	1·913	33·0	1·987
	21·0	2·801	53·86	2·664	29·425	4·358	32·86	2·079
JUNE.								
1848.	20·5	6·283	61·1	2·656	29·457	8·858	40·6	1·987
1849.	21·2	5·937	64·5	2·513	·455	7·803	43·3	1·938
1850.	21·6	2·614	62·2	2·323	·486	2·626	40·6	1·448
	21·1	4·944	62·60	2·497	29·466	6·429	41·5	1·791
JULY.								
1848.	20·2	2·776	64·8	2·598	29·502	3·638	44·6	2·096
1849.	21·8	1·898	65·5	2·461	·476	3·173	43·7	2·177
1851.	21·8	5·319	62·1	3·066	·390	8·484	40·3	2·900
1853.	21·0	3·343	65·1	2·236	·502	1·831	44·1	2·306
1854.	21·7	4·039	64·5	1·991	·440	5·189	42·8	1·916
1855.	22·0	2·980	63·9	2·171	·419	2·248	41·9	2·161
	21·41	3·392	64·31	2·420	29·454	4·094	42·90	2·259
AUGUST.								
1848.	19·6	3·468	63·2	2·704	29·492	1·953	44·2	2·109
1849.	20·3	0·980	61·7	2·326	·452	1·650	41·4	1·834
1851.	20·3	3·780	63·1	2·118	·477	3·906	42·8	2·312
1853.	19·7	2·516	64·8	1·910	·460	3·232	45·1	1·849
1854.	20·4	2·823	61·9	1·765	·512	4·764	41·5	1·407
	19·94	2·718	62·94	2·178	29·478	3·101	43·0	1·902

Table II. (*continued*).

Year.	Difference of Mean Temperatures of the two Stations.	Geneva. Rainfall.	Geneva. Mean Temperature.	Geneva. Amount of Barometric Oscillation.	Milan. Mean height of Barometer.	Great St. Bernard. Rainfall.	Great St. Bernard. Mean Temperature.	Great St. Bernard. Amount of Oscillation.
	SEPTEMBER.							
	°	in.	°	in.	in.	in.	°	in.
1848.	19·8	2·087	56·5	2·733	29·487	2·728	36·7	1·930
1851.	19·4	3·382	52·2	2·036	·542	5·728	32·8	1·981
1853.	19·2	4·571	56·6	2·821	·450	2·713	37·4	2·328
1854.	17·9	0·000	58·7	2·006	·647	0·031	40·8	1·811
1855.	20·6	4·346	59·5	2·732	·532	2·480	38·9	2·584
1858.	20·5	2·843	60·8	2·006	·553	1·728	40·3	1·750
	19·56	2·871	57·38	2·389	29·530	2·568	37·81	2·064
	OCTOBER.							
1849.	17·7	7·744	51·0	4·330	29·500	1·268	33·3	3·612
1851.	16·3	4·122	49·3	3·190	·499	3·327	33·0	2·793
1852.	17·8	6·512	48·4	3·625	·475	5·150	30·6	2·331
1856.	16·4	0·823	50·0	1·981	·706	3·339	33·6	1·715
	17·05	4·800	49·67	3·281	29·545	3·271	32·62	2·613
	NOVEMBER.							
1849.	14·9	1·724	36·8	4·059	29·453	5·748	21·9	3·909
1850.	17·5	2·992	42·7	3·728	·512	5·551	25·2	3·230
1852.	16·5	5·039	45·3	4·727	·412	1·827	28·8	3·333
1853.	17·8	1·405	41·7	2·915	·557	2·980	23·9	
1855.	18·1	2·453	39·3	2·824	·457	3·437	21·2	1·820
1857.	14·9	1·622	41·0	2·506	·612	1·165	26·1	2·357
1858.	17·9	3·110	37·3	3·467	·372	4·461	19·4	2·462
	16·80	2·620	40·58	3·461	29·482	3·595	23·78	2·852
	DECEMBER.							
1848.	11·4	1·272	33·4	3·554	29·754	3·583	22·0	2·810
1850.	13·3	0·933	34·5	2·972	·618	1·925	21·2	2·478
1851.	6·3	0·197	25·9	2·337	·756	0·106	19·6	2·231
1852.	12·6	1·539	37·9	4·666	·613	6·007	25·3	4·176
1857.	9·2	0·738	33·0	2·772	·889	0·331	23·8	2·351
	10·56	0·935	32·94	3·260	29·726	2·390	22·38	2·809

Table III.

Year.	Difference of Mean Temperatures of the two Stations.	Geneva. Rainfall.	Geneva. Mean Temperature.	Geneva. Amount of Barometric Oscillation.	Milan. Mean height of Barometer.	Great St. Bernard. Rainfall.	Great St. Bernard. Mean Temperature.	Great St. Bernard. Amount of Oscillation.
JANUARY.								
	°	in.	°	in.	in.	in.	°	in.
1849.	17·5	3·303	35·3	3·941	29·580	8·622	17·8	3·153
1853.	19·9	2·354	37·7	4·452	·483	6·283	17·8	3·346
1856.	18·0	4·968	36·3	5·349	·356	5·500	18·3	3·291
1857.	21·6	1·236	32·4	5·827	·237	1·047	10·8	3·891
	19·25	2·965	35·42	4·892	29·414	5·363	16·17	3·420
FEBRUARY.								
1848.	18·4	1·435	37·8	5·725	29·433	10·929	19·4	4·286
1852.	21·5	0·776	36·3	4·471	·422	4·961	14·8	3·550
1853.	24·9	0·799	31·5	4·727	·046	5·795	6·6	4·061
1854.	19·3	0·291	30·1	4·049	·529	0·961	10·8	3·463
1858.	19·0	0·736	33·0	2·945	·573	1·441	14·0	2·501
	20·62	0·807	33·74	4·383	29·400	4·817	13·12	3·572
MARCH.								
1848.	22·1	3·465	39·9	4·760	29·302	6·405	17·8	3·868
1851.	22·3	2·831	38·7	3·744	·395	5·862	16·4	2·689
1853.	21·0	0·654	32·8	3·566	·313	6·760	11·8	3·022
1855.	23·3	1·713	40·3	4·799	·191	3·193	17·0	3·304
1858.	20·7	1·079	38·8	4·003	·352	1·913	18·1	3·877
	21·88	1·948	38·10	4·174	29·311	4·826	16·22	3·352
APRIL.								
1849.	22·0	2·425	42·8	3·929	29·216	8·591	20·8	2·956
1853.	23·5	2·445	45·2	3·871	·349	7·024	21·7	2·734
1854.	22·0	0·846	49·5	3·634	·554	1·992	27·5	2·910
1856.	23·1	3·579	49·8	3·298	·369	5·421	26·7	2·852
1857.	22·4	1·724	45·3	4·171	·253	3·780	22·9	2·940
1858.	22·0	2·453	51·8	2·887	·387	2·039	29·8	2·880
	22·50	2·245	47·4	3·631	29·354	4·808	24·90	2·878

Table III. (*continued*).

Year.	Difference of Mean Temperatures of the two Stations.	Geneva. Rainfall.	Geneva. Mean Temperature.	Geneva. Amount of Barometric Oscillation.	Milan. Mean height of Barometer.	Great St. Bernard. Rainfall.	Great St. Bernard. Mean Temperature.	Great St. Bernard. Amount of Oscillation.
				MAY.				
	°	in.	°	in.	in.	in.	°	in.
1850.	22·4	4·445	51·6	3·108	29·382	4·890	29·2	1·938
1852.	23·5	2·248	55·3	2·315	·463	4·406	31·8	1·863
1854.	23·0	2·425	55·4	1·902	·375	5·374	32·4	1·941
1855.	23·6	2·882	52·0	3·906	·318	4·626	28·4	2·520
1856.	22·7	11·724	51·6	3·296	·330	11·287	28·9	2·822
1858.	22·5	3·264	52·3	3·515	·360	2·508	29·8	3·974
	22·95	4·498	53·03	3·007	29·371	5·515	30·08	2·509
				JUNE.				
1851.	22·9	0·124	62·5	2·042	29·578	0·921	40·6	1·664
1852.	22·8	3·913	59·7	1·754	·458	4·878	36·9	1·657
1853.	22·4	2·811	60·1	2·498	·354	3·268	37·7	2·397
1854.	22·9	4·929	60·1	2·892	·423	6·760	37·2	1·920
1855.	23·3	2·713	60·3	3·233	·452	4·950	37·0	2·098
1856.	22·6	2·890	62·2	2·838	·487	3·303	39·6	2·629
1857.	23·2	2·000	61·0	2·276	·457	2·953	37·8	2·309
1858.	22·8	0·665	66·4	1·285	·468	1·110	43·6	1·194
	22·86	2·505	61·66	2·354	29·460	3·518	38·8	1·983
				JULY.				
1850.	22·2	0·142	63·9	1·789	29·444	1·425	41·7	1·889
1852.	22·3	2·335	66·4	2·181	·458	2·169	44·1	1·850
1856.	22·8	2·728	64·1	2·414	·423	2·535	41·3	1·842
1857.	23·5	0·734	68·8	1·549	·485	0·028	45·3	1·728
1858.	23·3	5·504	62·4	3·104	·347	3·772	39·1	2·438
	22·82	2·288	65·12	2·207	29·431	1·985	42·30	1·949
				AUGUST.				
1850.	21·5	3·374	62·5	2·586	29·491	4·280	41·0	2·041
1852.	21·3	8·437	61·9	2·316	·439	4·665	40·6	2·200
1855.	21·8	0·224	66·3	1·727	·486	1·819	44·5	1·638
1856.	21·8	2·362	67·9	2·482	·398	2·252	46·1	2·268
1857.	22·1	3·543	64·8	1·917	·416	3·185	42·7	2·110
1858.	21·8	3·539	60·9	2·173	·386	2·327	39·1	2·090
	21·71	3·579	64·05	2·200	29·436	3·088	42·33	2·058

TABLE III. (*continued*).

Year.	Difference of Mean Temperatures of the two Stations.	Geneva.			Milan.	Great St. Bernard.		
		Rainfall.	Mean Temperature.	Amount of Barometric Oscillation.	Mean height of Barometer.	Rainfall.	Mean Temperature.	Amount of Oscillation.
SEPTEMBER.								
	°	in.	°	in.	in.	in.	°	in.
1849.	21·4	3·670	58·3	2·876	29·487	2·839	36·9	1·979
1850.	22·3	3·075	54·8	2·162	·577	1·382	32·5	1·795
1852.	21·3	7·343	56·9	2·896	·482	4·350	35·6	1·808
1856.	23·1	4·598	55·9	3·490	·377	3·555	32·8	2·430
1857.	21·1	2·397	60·8	2·233	·520	2·398	39·7	2·054
	21·84	4·216	57·34	2·731	29·480	2·905	35·50	2·013
OCTOBER.								
1848.	19·9	3·398	49·0	3·992	29·444	5·614	29·1	3·005
1850.	20·2	2·378	45·5	3·766	·284	4·272	25·3	3·695
1853.	19·6	5·181	49·5	3·661	·480	6·783	29·9	2·633
1854.	19·3	4·236	50·5	4·156	·497	4·161	31·2	2·833
1855.	19·0	10·957	52·3	3·639	·370	12·524	33·3	2·509
1857.	19·8	3·228	51·3	3·632	·446	5·228	31·5	2·867
1858.	19·4	2·882	50·7	3·223	·439	0·764	31·3	2·126
	19·6	4·608	49·83	3·724	29·422	5·621	30·23	2·809
NOVEMBER.								
1848.	19·2	1·657	36·9	5·440	29·511	4·031	17·7	3·700
1851.	22·9	1·256	32·5	4·184	·275	3·106	9·6	3·237
1854.	20·5	3·098	38·0	3·257	·280	4·551	17·5	2·934
1856.	19·0	1·220	36·0	4·032	·408	0·996	17·0	3·079
	20·4	1·808	35·85	4·228	29·368	3·171	15·45	3·237
DECEMBER.								
1849.	18·8	0·909	31·6	4·406	29·431	4·555	12·8	3·248
1853.	16·5	0·504	28·8	3·578	·382	2·504	12·3	3·022
1854.	21·5	2·043	36·6	5·090	·395	2·350	15·1	5·366
1855.	15·4	1·240	27·0	4·789	·485	1·413	11·6	3·224
1856.	16·5	2·463	33·7	5·903	·468	3·520	17·2	5·701
	17·74	1·431	31·54	4·753	29·432	2·868	13·8	4·112

Table IV.

	Difference of Mean Temperatures of the two Stations.	Geneva. Rainfall.	Geneva. Mean Temperature.	Geneva. Amount of Barometric Oscillation.	Milan. Mean height of Barometer.	Great St. Bernard. Rainfall.	Great St. Bernard. Mean Temperature.	Great St. Bernard. Amount of Oscillation.
	°	in.	°	in.	in.	in.	°	in.
Jan. ...	15·53	1·242	29·98	3·842	29·567	3·470	14·45	3·234
Feb. ...	16·18	1·748	35·76	3·264	·572	2·702	19·58	2·561
March ..	18·73	0·854	38·45	3·543	·542	1·536	19·71	3·048
April ...	21·18	2·911	47·32	3·415	·384	6·330	26·14	2·489
May ...	21·00	2·801	53·86	2·664	·425	4·358	32·86	2·079
June ...	21·10	4·944	62·60	2·497	·466	6·429	41·50	1·791
July ...	21·41	3·392	64·31	2·420	·454	4·094	42·90	2·259
August..	19·94	2·718	62·94	2·178	·478	3·101	43·00	1·902
Sept. ...	19·56	2·871	57·38	2·389	·530	2·586	37·81	2·064
Oct. ...	17·05	4·800	49·67	3·281	·545	3·271	32·62	2·613
Nov. ...	16·80	2·620	40·58	3·461	·482	3·595	23·78	2·852
Dec. ...	10·56	0·935	32·94	3·260	·726	2·390	22·38	2·809
	18·25	31·836	47·98	36·214	29·514	43·862	29·72	29·701

Table V.

	Difference of Mean Temperatures of the two Stations.	Geneva. Rainfall.	Geneva. Mean Temperature.	Geneva. Amount of Barometric Oscillation.	Milan. Mean height of Barometer.	Great St. Bernard. Rainfall.	Great St. Bernard. Mean Temperature.	Great St. Bernard. Amount of Oscillation.
Jan. ...	19·25	2·965	35·42	4·892	29·414	5·363	16·17	3·420
Feb. ...	20·62	0·807	33·74	4·383	·400	4·817	13·12	3·572
March ..	21·88	1·948	38·10	4·174	·311	4·826	16·22	3·352
April ...	22·50	2·245	47·40	3·631	·354	4·808	24·90	2·878
May ...	22·95	4·498	53·03	3·007	·371	5·515	30·08	2·509
June ...	22·86	2·505	61·66	2·354	·460	3·518	38·80	1·983
July ...	22·82	2·288	65·12	2·207	·431	1·985	42·30	1·949
August..	21·71	3·579	64·05	2·200	·436	3·088	42·33	2·058
Sept. ...	21·84	4·216	57·34	2·731	·480	2·905	35·50	2·013
Oct. ...	19·60	4·608	49·83	3·724	·422	5·621	30·23	2·809
Nov. ...	20·40	1·808	35·85	4·228	·368	3·171	15·45	3·237
Dec. ...	17·74	1·431	31·54	4·753	·432	2·868	13·80	4·112
	21·18	32·898	47·75	42·284	29·406	48·485	26·57	33·892

XIII.—*Memoir of the late* Eaton Hodgkinson, *F.R.S., F.G.S., M.R.I.A., Hon. Mem. R.I.B.A., Inst. C.E., Roy. Scot. Soc. Arts, and Soc. Civ. Eng. Paris, Prof. of the Mech. Princ. of Engineering, University College, London.* By Robert Rawson, Esq., Honorary Member of the Society.

Read March 4th and October 7th, 1862.

The subject of this memoir was born, of respectable parents, at the small village of Anderton, in the parish of Great Budworth, Cheshire, on the 26th of February 1789; died at Eglesfield House, Higher Broughton, Manchester, June 18th, 1861, in his 72nd year, and was interred at his native village. His father died when he was about six years of age, leaving his widow with three children, whose education and maintenance depended upon her exertions and prudence.

He left his native village, with his mother and sister, at the age of twenty-two, and came to reside at Salford, Manchester, where he remained the greater portion of his after-life.

He was elected a member of this Society in the year 1826, and he enriched the Society's memoirs with the following important papers, thus laying the foundation of his reputation as a sound mathematician and an original thinker:—

"On the Transverse Strain and Strength of Materials" (read March 22nd, 1822), vol. iv.

"On the Chain Bridge at Broughton" (read Feb. 8th, 1828), vol. v.

"On the Forms of the Catenary in Suspension Bridges" (read Feb. 8th, 1828), vol. v.

"A few Remarks on the Menai Bridge" (read Dec. 12th, 1828), vol. v.

"Theoretical and Experimental Researches to ascertain the Strength and best Forms of Iron Beams" (read April 2nd, 1830), vol. v.

"Appendix to the Paper on the Chain Bridge at Higher Broughton, Manchester," vol. v.

"Some account of the late Mr. Ewart's paper on the Measure of Moving Force, and of the recent applications of the principle of Living Forces to estimate the effects of Machines and Movers" (read April 30th, 1844), vol. vii.

He occupied in succession the distinguished positions of Vice-President and President of this Society.

He was a leading member of the British Association for the Advancement of Science from its commencement, and contributed greatly to the interest and efficiency of the mathematical and mechanical sections. He also gave active help to the Association in several valuable reports on pure and mixed science. These reports, which have in a great degree assisted in maintaining the high scientific renown of the Association, are as follows:—

Third Report, 1833:—"On the Effect of Impact on Beams."—"On the direct Tensile Strength of Cast Iron."

Fourth Report, 1835:—"On the Collision of Imperfectly Elastic Bodies."

Fifth Report, 1835:—"Impact upon Beams."

He held the distinguished position of Vice-President of the Association in the year 1861.

In the year 1841 he was elected a Fellow of the Royal Society, and contributed to its Transactions two elaborate papers:—

"Experimental Researches on the Strength of Pillars of Cast Iron and other materials" (read May 14th, 1840).

The aim of this paper was greatly extended in the second communication:—

"Experimental Researches on the Strength of Pillars of Cast Iron from various parts of the Kingdom" (read June 1857).

For the first paper the Council of the Royal Society awarded the gold medal, as a mark of their appreciation of its practical investigations.

He was appointed Professor of the Mechanical Principles of Engineering at University College, London, on the 6th of February, 1847, and lectured during the sessions of 1847 to 1853 inclusive.

In 1847 he was appointed a Member of the Royal Commission to inquire into the Properties of Wrought and Cast Iron, and their application to Railway Structures. The results of his labours in this important inquiry are given, with marked reference to their magnitude and efficiency, in the Commissioners' Report of 1849.

He was consulted by the late Robert Stephenson in reference to the construction of that great national work, the Tubular Bridge over the Menai Straits. His experience and mathematical knowledge enabled him to suggest and carry out a series of experiments, at the cost of several thousand pounds, with a view to investigate the bearing-properties of wrought-iron riveted tubes, and to satisfy the mind of this great engineer as to the stability and safety of the Britannia and Conway Tubular Bridges.

He edited an edition of 'Tredgold on Cast-iron,' to which he added a second volume, giving an account of his own experiments and discoveries; published by Weale, 1846.

The title of the second volume is, 'Experimental Researches on the Strength and other Properties of Cast Iron, with the development of New Principles, calculations deduced from them, and inquiries applicable to Rigid and Tenacious Bodies generally.'

The most novel and important conclusions here given are as follows:—

The strengths of long pillars of cast iron, wrought iron, cast steel, and Dantzic oak, of the same dimensions, are in proportion to the numbers 1000, 1745, 2518, 109. Cast iron is not reduced in strength when its temperature is raised to 600°.

The sets, in cast-iron beams, vary nearly as the square of the force of deflection; hence any force, however small, will injure the elasticity of cast iron. The strength in tons of beams approaching the best form is measured by the formula, $2 \cdot 166ad \div l$, where $a =$ area of section of bottom flange in the middle, $d =$ the depth in inches of the beam, and $l =$ the distance in feet.

A general investigation of the position of the neutral line is given on the principle that the forces of extension and compression of a particle vary as any function of its distance from the neutral line. This includes every hypothesis which has been proposed in order to compute the strength of material bodies subjected to strains.

Birth and Education.

As I have already stated, Mr. Hodgkinson was born at Anderton, Cheshire, in the year 1789. His father, a respectable farmer, died of fever when his son Eaton was about six years of age, leaving Mrs. Hodgkinson with two daughters and a son.

On his father's decease, his mother determined to continue the farm; and by industry, thriftiness, and business-like habits she was enabled to educate her children respectably, and to send her son to the Grammar School of Northwich.

At this school he received the rudiments of a classical education, as he studied the Latin, Greek, and Hebrew

languages under the immediate supervision of the head master, Mr. Littler. This was done to meet the wish of his uncle, the Rev. Henry Hodgkinson, Rector of Aberfield, Berkshire, who was very anxious that his nephew should be educated with a view of going to Oxford or Cambridge, to prepare for the Church. The desire of his uncle was, for a time, gratified, and the hope was strongly indulged that one day Eaton Hodgkinson would be a student of one of the universities; hence the study of classics in his early youth was considered indispensable, although it was not exactly in conformity with his tastes and habits of thought, as at an early age he was naturally more inclined to the study of mathematics than of languages.

To the severe treatment which he here suffered, his cousin, Mrs. Thompson, attributes the nervous tremor of his hands and speech which continued with him through life, and was a serious impediment to his success. The Rev. Mr. Littler was a very severe disciplinarian; and if a boy could not learn, he tried to flog it into him; and young Hodgkinson, owing to his inaptitude for languages, having received a sound thrashing for not having learned his lessons perfectly, was removed from the grammar school and placed in a private school in Northwich, of far less pretensions, but more in unison with his aspirations.

This private school, to which he was removed because he did not show a decided taste for the study of languages, was conducted by Mr. Shaw, a gentleman of superior mathematical attainments, and possessing great tact in teaching and in the general management of boys. It was at this school that Mr. Hodgkinson finished his youthful education. He obtained a good degree amongst his school-fellows, and a distinguished position in the affections of his master. The instructions of Mr. Shaw in mathematical subjects were fully appreciated by Hodgkinson, and con-

sequently he made rapid advances in the various studies to which his attention was directed. Here he laid the foundation of that mathematical knowledge which he afterwards applied with singular success to the extension and development of the theory and practice of the strength of materials. The bias of Mr. Hodgkinson's mind at this period, and the position in which his mother was left, seemed to require a reconsideration of his future. He was now growing in stature as well as in knowledge, and his mother found him very useful to her in the outdoor work on the farm; therefore it was deemed desirable to abandon the idea, once strongly entertained, of prosecuting her son's education with a view of entering the Church, and to allow him to devote his attention and energies to the skilful management of farming.

Mr. Hodgkinson therefore gave up all thoughts of the Church; and Latin, Greek, and Hebrew were changed for more congenial subjects of study. He commenced at once his career as a Cheshire farmer; but although he felt it a duty to assist his dear mother, and meet her wishes to the best of his ability, still he made but little progress in his new vocation. Farming, which had been thrust upon him by sheer necessity, was not suited to his genius; but he pursued it for a time as a paramount duty, from which his conscientious devotedness to his mother and sisters would not allow him to escape. The seeds of pure and mixed science, which had been thrown broadcast into his youthful mind by Mr. Shaw, were now beginning to germinate, and to rise from their latent state into full and sensible existence, creating, as they advanced to maturity, new wants and fresh desires, which could not be gratified by farming or the society of a Cheshire village. The fruit thus developed at the village school indicated, with unerring certainty, a different direction from Cheshire farming or the Church. His mother saw this, and she was ready to bend to circum-

stances which she could not successfully resist. Hence he persuaded her to give up her farm in Cheshire, and embark her small capital in a pawnbroking business at Salford, Manchester. Their friends advised this step, as the best to promote the interests of the family, and satisfy the thirst of Mr. Hodgkinson for scientific knowledge and society. The family therefore moved from Great Budworth, Cheshire, to Salford, Manchester, in the year 1811, when Mr. Hodgkinson was about twenty-two years of age. This step was the turning-point of his career; and, but for this, in all probability he would have past a life of inglorious ease in a Cheshire village, unknown as a cultivator of mathematical and physical science.

His residence in Manchester was soon productive of important consequences; his habits of thought became fixed, and the line of scientific inquiry in which he was to advance was not long left indeterminate. Manchester at this period was in its youthful vigour; it contained men of great intellectual endowments, each anxious to distinguish himself in some department of useful knowledge: amongst these the names of Dalton, Henry, and several others stand out preeminent.

The business, under the control and management of Mrs. Hodgkinson, assisted by her son and daughter, was successful.

Mr. Hodgkinson's spare moments from business were now entirely devoted to reading any standard works on science which he could procure. The works of Simpson, Emerson, and Dealtry contributed greatly to his knowledge. He read these authors with earnestness and fidelity, and was wholly indebted to them for his knowledge of the higher departments of mathematical research. Many of the self-taught men of the last and the beginning of the present century have expressed their obligations to Thomas Simpson and William Emerson. Their works, whatever prejudice

may think or say to the contrary, were the best standard works of the age; and it may be affirmed that the scientific literature of the 18th century is accurately and faithfully reflected from the pages of the weaver of Market Bosworth, Thomas Simpson, and the Hurworth village schoolmaster, William Emerson.

These humble but highly gifted men were more catholic in their writings than are the authors of the present age. They wrote to instruct the mass of mankind; but the writers in these days labour for a special purpose, which is limited in its operation: they write only to supply the daily routine of the school, without casting a single thought beyond its boundary.

The late Rev. Robert Murphy, a Cambridge mathematician of distinguished eminence, speaks of Thomas Simpson as an analysist of first-rate genius (see 'Murphy's Equations'). M. Clairaut, when in England, paid Simpson a visit at Woolwich, in order to compare his own investigations on the motion of the moon's apogee with the investigations of Simpson on the same subject.

This fact alone shows the high position in which Simpson stood in the estimation of the most eminent mathematicians of Europe.

In consequence of his ardent love for scientific pursuits, Mr. Hodgkinson became acquainted with the most gifted men then living in Manchester. Dr. Dalton, Holme, Henry, Ewart, Sibson, Johns, Fairbairn, were amongst the scientific friends with whom he could freely converse on subjects which possessed a mutual interest. In his mathematical reading he sought and obtained the help of Dr. Dalton, who was then a private teacher of mathematics in Manchester. He became one of Dalton's pupils, and read with him the works of Lagrange, Laplace, Euler, and Bernouilli, whose writings were now engaging the attention of the best and foremost mathematicians of England.

These authors had been instrumental in producing a great change in the mathematical sciences at Cambridge; their investigations were models of elegant algebraical demonstration, both with regard to symmetry of notation and subject-matter of inquiry. The friendship of Dalton and Hodgkinson, cemented by genial minds and kindred pursuits, continued uninterruptedly till the death of Dalton. Though each of these men had his distinctive field of labour, yet each could hold converse with the other on their respective researches, and Mr. Hodgkinson entertained through life a profound respect for the high character and great chemical discoveries of his friend. The extent of his mathematical reading at this period may be estimated by referring to his paper entitled "On the Transverse Strain and Strength of Materials," printed in the Fourth Volume of this Society's Memoirs.

His Character.

The late Professor Hodgkinson, like a true philosopher, was satisfied with a small but adequate competency, and, retiring from business at an early period, he devoted a long life and rare mental gifts to the development of science. And it is a pleasing reflection, that while many men very eminent in the history of science have had to wait a long time before their discoveries have been recognized and adopted, Mr. Hodgkinson had the unusual pleasure of seeing the fruits of his labours appreciated and applied to the construction of great practical engineering enterprises. The youthful days of Mr. Hodgkinson were not marked by precocious talents and wonderful achievements; still he possessed, even in youth, a quick perception of the relations of abstract magnitudes, and manifested, like Newton and Stephenson, a strong propensity for making sun-dials.

Manhood developed in him a profound intellect, a highly

cultivated intelligence, unwearied perseverance, and a kind and an affectionate heart. He discharged every relation of life with fidelity, and has left behind him a name great in the annals of science, reflecting every manly virtue, and unsullied by any act of meanness. He was, however, very jealous of the products of his own mental labours, which he regarded as personal property; and was also equally just in the use of the mental property of other cultivators of science, as he would not appropriate the conclusions of any man without due acknowledgment.

If he did entertain any hostile feeling, it was against those who, as he conceived, were unscrupulous in their appropriation of the fruit of other men's brains. His sense of justice would not allow him to show the slightest sympathy with this class of offenders.

The efficiency of Mr. Hodgkinson's lectures at University College, and of his oral instructiou generally, was somewhat circumscribed by his hesitancy of speech. This peculiarity interfered with his usefulness as a speaker and teacher, and rendered his explanations of subjects, even those with which he was most familiar, somewhat tedious to the student. And it is perhaps one of the greatest evidences that can be recorded of the power of his mind, that he was thought worthy, in spite of his embarrassed address and slowness of speech, to be installed in a professorial chair in one of the leading scientific colleges of the kingdom. As a relaxation from severe mental toil, he cultivated a taste for general literature and the architecture of the middle ages. Of late years he frequently travelled, both on the Continent and in the British Empire, to examine those stupendous cathedrals and other public buildings which adorn Western Europe, and which testify to the good taste, piety, and intellectual culture of the age in which they were built. He was fond also of investigating the remains of antiquity. And, what is valued

above all by a man of science, he enjoyed the friendship and esteem of his contemporaries, who were able to estimate his worth, appreciate his talents, and apply his discoveries to useful purposes. The most eminent engineers of the age placed unbounded confidence in the results of his experiments, believing them to be faithfully recorded, and accurately reduced to meet the requirements of mathematical formulæ. As a confirmation of this, it may be stated that the engineers' pocket- and text-books of the present time are full of Hodgkinson's formulæ for calculating the strength and deflection of pillars and beams.

Mr. Hodgkinson was twice married, but without issue in each case. His first wife was Miss Catherine Johns, daughter of the respected Rev. William Johns, a distinguished Member of this Society, who contributed an interesting paper to its memoirs, entitled "Remarks on the Use and Origin of Figurative Language" (vol. ii. new series). His second wife was Miss Holditch, daughter of Henry Holditch, Esq., Captain in the Cheshire Militia. This lady, who is now left to mourn her loss, devoted her powers to comfort and sustain her husband when his health and memory would not admit of his having recourse to his favourite pursuits. Of late his great mental powers became prostrate, and his memory failed so much that it was obvious to his friends the time had arrived when his faculties required repose. In this state of mental lassitude the services of Mrs. Hodgkinson were of great value to him. It is not unusual with men whose mental powers have been overstrained by excitement and hard labour, that the desire for intellectual activity does not cease when the physical power necessary to sustain it is feeble. Mr. Hodgkinson was the subject of this painful experience: the desire for mental activity continued unabated to the last; and it was only a few months before his decease that he was engaged in arranging his papers, with a view to publish them, so

that they might be more accessible to engineers than they now are in the volumes of learned societies.

Mr. Hodgkinson's religious emotions were silent, devotional in the highest sense—not sectarian; they were strictly confined to the channel between his Maker and his own soul. And in this way they were purified by the truth from heaven, bearing the precious fruit of meekness, charity, and implicit confidence in Him who is all and sustains all. His religion was the arbiter of his life, the judge of the many and important obligations between God, his fellowman, and himself. His end was peaceful, and he has left a name marked by strict integrity, which will be well remembered in the walks of science for ages yet to come.

Let us now pass on to notice more in detail the works of Mr. Hodgkinson, which have raised him to a good degree amongst his cotemporaries, and will also be the introduction to future thinkers in the same field of labour which he successfully cultivated.

"On the Transverse Strain and Strength of Metals" (read March 22nd, 1822), vol. iv.

The objects aimed at in this paper are, as stated by the author, to unite, in a general formula, the commonly received theories, in which all the fibres are conceived to be in a state of tension; and next, to adapt the investigation to the more general case, where part of the fibres are extended and part compressed, and to seek experimentally for the laws that regulate both the extensions and compressions. The manner in which these objects have been sought and developed is a model, worthy of every commendation, of clear, sound, geometrical reasoning and refined artifice. And the data necessary to give practical effect to the various analytical formulæ have been obtained from experiments, than which none have been recorded with

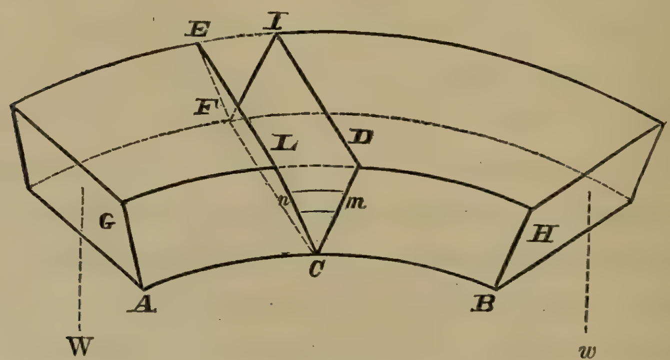
greater fidelity and less contortion to meet the demands of particular theories. No painstaking or expense was considered too great to make the results of the experiments successful and trustworthy, so that the engineer and philosopher alike could place implicit reliance upon them. In these experiments there is recorded, for the first time, an element which has furnished a theme for many animated discussions of late years amongst philosophers and practical engineers, and which became an important object of research in all Mr. Hodgkinson's subsequent experiments, viz. *set*, or the difference between the original position of a strained body and the position which it assumes when the strain is removed.

This point, which is full of interest and important consequences to the practical man, cannot now be discussed. On examination, I believe that I shall be borne out in the statement that, notwithstanding the number of books which have been written during the last thirty years on the strength and strain of materials, some of a more ambitious kind, and others having the humbler object of being useful in communicating information to the artisan, still there is none from which a clearer and more satisfactory exposition of this subject can be gathered than from the paper above referred to, by Mr. Hodgkinson in the vol. for 1822. The Tuscan Philosopher, Galileo, has the merit of first propounding a theory of the strength of materials, and applying the unerring principles of geometry to the computation of the strength of beams of given dimensions. With Leibnitz originated the idea of the force of extension of a fibre being proportional to its distance from the lower side of a bent beam. James Bernouilli first suggested the notion (for it never assumed any other shape in his mind) of a *neutral line* in the section of rupture. But to the late Professor Hodgkinson belonged the merit of giving practical effect, in this paper, to the happy suggestion of Ber-

nouilli, by showing, both theoretically and experimentally, the true method of determining, in the section of fracture, the exact position of the neutral line, and of calculating the strength of the beam.

In order, then, to show more clearly the steps taken by Mr. Hodgkinson in the establishment of sound practical views of this subject, it will be necessary to give a brief exposition, by reference to diagrams, of the history of the section of fracture.

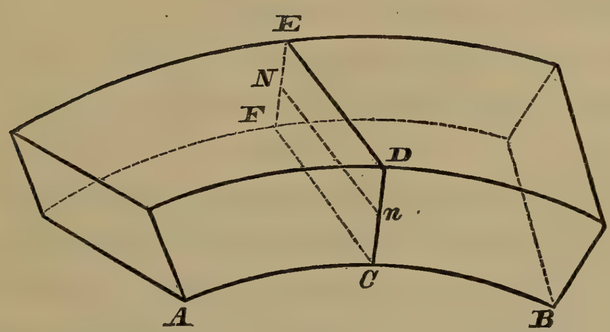
Fig. 1.



Every beam of material is supposed to be composed of an indefinite number of parallel filaments in the direction of its length, and the breaking of the beam necessitates the breaking of each of the filaments of which it is composed. Suppose a beam, AB, placed on the fulcrum CF, to be broken at the section CDIEFL by the weights W and *w*, suspended from the points G and H. The theory of Galileo assumes the beam to turn about the fulcrum CF, and each of the filaments (*nm*) in the section of fracture CLD to sustain an equal force. This hypothesis implies the incompressibility and inextensibility of the material. Leibnitz, the great rival of Newton, assumed the beam to turn about the fulcrum, CF, as did Galileo, but assumed, what experiment has confirmed, the force of each filament in the section of rupture to vary in proportion to its distance from the fulcrum, CF. This theory implied the incom-

pressibility and extensibility of the material. James Bernouilli, to whose labours science is under great and varied obligations, in reviewing the theories of Galileo and Leibnitz, felt convinced of their incorrectness, and that, of course, they could have no favourable response from the voice of nature. He assumed, or rather suggested, the beam (fig. 2) to turn about the line N*n*, which is now com-

Fig. 2.



monly called the neutral line, and the filaments above N*n* to be extended, and those below N*n* to be compressed. He supposed, with Leibnitz, the force of each filament to be proportional to its distance from N*n*, the neutral line.

This theory clearly indicates the compressibility and extensibility of the material, and so far agrees with all recorded experience. Bernouilli never pushed this idea beyond a suggestion, and it remained unfruitful in consequences till Mr. Hodgkinson, in this paper, followed it up, and fixed, for the first time, the exact position of the neutral line, and made it subservient to the computation of the strength of a beam of given dimensions. We can scarcely exaggerate the importance of this step, as it forms the connecting link between the correct assumption of Leibnitz and the complete theory of the transverse strength of beams. Without the position of the neutral line, the section of fracture appeared dark and uncertain, and we now wonder that the determination of this line had escaped

the penetration of Professor Barlow and others who had examined the subject. The opinions here expressed are founded upon the results obtained by reading the works of the best authors previous to 1822.

Dr. Robison, Playfair, Barlow, Dr. O. Gregory, and Sir J. Leslie are sufficiently known in the walks of science, to justify the assertion that their works on elementary subjects represent the true state and progress of the knowledge of the strength and strain of materials. Playfair's 'Outlines of Natural Philosophy,' a work of great merit, and well adapted for the time in which it appeared, contains only the following paragraph on the subject of the *neutral line*:—

"But it is also said that a tube of metal has been found to support a greater transverse strain than a solid cylinder of the same diameter; or that a cylinder, when bored in the direction of the axis, and a considerable part taken away, was stronger than before." "This must undoubtedly arise from a change taking place in the position of the fulcrum or hinge round which the fracture is made. In the case of a cylinder, and indeed of all solids, the fulcrum is not the mere outward edge, but a point in the interior, on the one side of which the fibres are elongated, and on the other crushed together. The point, then, which serves as the fulcrum will be found within the solid, at a greater or less distance, as the parts resist lengthening more than crushing. The consequence of this is, that when the centre of gravity and the fulcrum are brought nearer to one another, the strength of the beam or bar is diminished. When the heart of a solid mass is cut out, as is supposed of the cylinder, the fulcrum, or the axis of the fracture, is perhaps kept nearer to the surface than when the whole is a solid mass. This, at least, seems to be the most probable account that can at present be given of a phenomenon not a little paradoxical, and not yet suffi-

ciently examined." (See Playfair's 'Outlines,' vol. i. p. 153.) Prof. Barlow, in his 'Essay on the Strength and Stress of Timber,' published in 1817, at p. 32, shows in an admirable manner the inaccurate views of Dr. Robison respecting the determination of the neutral line, but fails entirely to remedy the defect. Barlow proposes, what is equally ineffective, to fix the position of the neutral line, by supposing the moments of the extended fibres about the neutral axis to be equal to the moments of the compressed fibres about the same line." Sir J. Leslie, in his 'Elements of Natural Philosophy,' published in 1823, at p. 234, states that "in the case of a horizontal beam supported at both ends, but depressed by its own weight, the upper surface becomes concave, and the under surface convex. The particles of the upper surface are therefore mutually condensed; in a certain intermediate curve the particles are not affected longitudinally, though bent from their rectilineal position. This curve of neutral action may be assumed in the middle of the beam." Dr. O. Gregory, in his 'Mechanics,' published in 1826, at p. 122, vol. i., states, in reference to the subject of the neutral line, "There is, moreover, the consideration that, when a beam deposited horizontally, or nearly so, is ruptured by a vertical pressure, a horizontal stratum, from end to end, is compressed, and the other portion extended or stretched, the thin lamina between these two being regarded as a neutral axis; this again is a curious topic of inquiry." This author gives several theories of the strength of materials from Venturoli, not any one of which contains the correct determination of the neutral line. From these quotations of the best-informed writers, are we not justified in the inference that to the late Prof. Hodgkinson belonged the merit of first accurately conceiving the true mechanical principle by which the position of the neutral line, in the section of fracture, could be determined? He

did this by equating the forces of extension with the forces of compression—a method which is now universally adopted in computing the strength of beams. This method of fixing the neutral line, like all new methods, advanced to its present position by slow degrees; but, after many conflicts and discussions, the triumphant declaration of Prof. Barlow, at the British Association of 1833, established this great principle, which was first conceived by Mr. Hodgkinson, who, single-handed, had maintained his position against the formidable powers of acknowledged authorities. Prof. Barlow, in his report "on the Present State of our Knowledge respecting the Strength of Materials," printed in the third volume of the Reports of the British Association for the Advancement of Science, 1833, very justly alludes to this subject, and states as follows:—"Mr. Hodgkinson, however, in a very ingenious paper read at the Manchester Philosophical Society in 1822, has pointed out an error in my investigation, by my having assumed the momentum of the forces on each side of the neutral axis equal to each other, instead of the forces themselves. This paper did not come to my knowledge till the third edition of my essay was nearly printed off, and the correction could not then be made." The Rev. Dr. Whewell, in his 'Analytical Statics,' refers to this paper, and gives the investigation of the neutral line on the same principle as that adopted by Mr. Hodgkinson, who always maintained that "we could see no cause why it should be rejected, especially since it seems to us to be everywhere consistent and just." (See Manchester Phil. Society's Memoirs, vol. iv. p. 241.) It appears that his friend, Dr. Dalton, took great interest in the deductions of this paper, and discussed them freely with him as he proceeded with his experiments, which will ever be regarded as marking an epoch in the subject of the strength of materials. Indeed the theoretical investigations of this paper, though new and im-

portant, form only a small portion of its merits. The experiments recorded in it established the laws, "that the extensions of the fibres of a bent beam were proportional to the forces during the early stages of flexure; but as the extensions arrived nearer to fracture, they increased faster than the forces;" and, "that so long as the forces are moderate, and are applied in the direction of the fibres, the compressions will be as the forces; but when the beam becomes bent, the fibres, being then crushed, offer a feeble resistance to the force." These results were obtained direct from the unerring voice of nature. The first of these laws was announced by Dr. Robison, as a general law of nature, on the simple authority of a few experiments on the slips of gum caoutchouc and the juice of the berries of the White Bryony, of which a single grain will draw to a thread two feet long, and again return to a perfectly round sphere. (See 'Manchester Memoirs,' vol. iv. p. 252.)

"On the Forms of the Catenary in Suspension Bridges" (read February 8th, 1828, vol. v.).

The Chain-bridge at Broughton, Manchester, which broke down by a troop of soldiers marching over it, and the celebrated Menai suspension bridge, built by Telford, had stimulated inquiries respecting the best form of such structures. These inquiries, naturally enough, led to a reconsideration of the catenary, a curve the properties of which, under given conditions, were first discovered by James Bernouilli. (See Leslie's 'Geometry of Curved Lines.')

In this paper a great degree of generality is given to the catenarian curve. After the known properties of the common catenary are clearly investigated, the formulæ are then applied with great ability to determine the form of suspension bridges when the *weight of catenarian chain, the weight of the roadway*, and *the weight of the suspension-*

M 2

rods are taken into account. The introduction of these complex, though necessary, elements into the question, led to the formation of the following difficult and comprehensive differential equation :—

$$\frac{adx}{dy}=bz+cy+e\int xdy \quad . \quad . \quad . \quad . \quad (A),$$

where x, y, are the current coordinates of a point in the curve, and z the length of the curve from this point to the lowest point. The explanation of the constants a, b, c, e is as follows :—

a = the tension of the curve at its lowest point.

b = the weight of a unit of length of the curve.

c = the weight of a unit of length in the roadway, which is supposed to be divided transversely into separate parts, and may include any weight uniformly distributed over it, with that of the suspension-rods below the horizontal line.

e = the weight of a unit of vertical surface in the suspending-rods, the rods being here supposed to be uniformly distributed, and indefinitely near to one another, and therefore reckoned as a uniform surface.

This differential equation, under given conditions of the constants, is treated in this paper in a very able manner, showing great command over the resources of modern analysis, and facility in the use of the varied artifices employed in the integration of differential equations. The results arrived at have been referred to by the ablest writers of the age, Dr. Whewell and the Rev. Canon Moseley,—by the former in his 'Analytical Statics,' where the solution of equation (A), as given by Mr. Hodgkinson, occupies a distinguished place; by the latter in his 'Engineering and Architecture,' in which the labours of our late friend are honourably mentioned :—

"This problem appears first to have been investigated

by Mr. Hodgkinson in the fifth volume of the 'Manchester Memoirs;' his investigation extends to the case in which the influence of the weights of the suspending-rods is included." After such testimony, it would be presumption on my part to enter more into detail on this paper. To a modern student, however, the notation and procedure adopted may possibly contrast unfavourably with the notation and procedure which characterize the elementary works of the present day. To such student, if there be any, I would suggest that in forming an opinion on a paper like this, written more than thirty years ago, it would be unfair to exclude the comparison of the state of mathematical and physical science at that period with the present. It must be remembered that Lord Brougham and his coadjutors in a great work have done much to popularize and spread amongst their countrymen a knowledge of the arts and sciences. These interesting subjects can now be read as they have come from the hands of Euler, Lagrange, and Laplace, by means of cheap publications, which are within the reach of the humblest artisan. In consequence of this, it is not high praise to state that questions in mathematics which could have been accomplished with difficulty thirty years ago can now be readily solved by the present methods, which are now extensively known amongst the youth of all ranks in society though the warming stimulant of competitive examinations. In this statement, I am anxious not to be misunderstood, and to guard against giving an opinion as to the question, "Has mathematical power increased in the degree commensurate with the increase of mathematical learning?" This will form a nice question for the future historian of the inductive sciences to determine. I may, however, express my views on this debatable question so far as to say that I have but little confidence in the products of unnatural growth of any kind.

There is a very marked difference in the mathematics of this and his former paper "On the Strength of Materials." The great battle between the *dots* and the *d's* had been fought at Cambridge University with earnestness on both sides, and, chiefly through the invincible courage and inexhaustible armoury of Woodhouse, Peacock, Babbage, and Herschel, the *d's* of Leibnitz wrested the victory from the *dots* of Newton. The effects of this victory, which has produced a great change in the mathematical literature of this country, are clearly seen in this paper, the principles investigated in which are applied to the numerical computation of the strength and strains of the Menai and Broughton Suspension-bridges.

"Theoretical and Experimental Researches to ascertain the Strength and best Forms of Iron Beams" (read April 2nd, 1830).

Whether we consider the theoretical exposition of the section of fracture, or the faithfully recorded experiments and their practical deductions, we must regard this paper as the most valuable and original contribution to the history of the strength of materials which this century can boast. There is no work in our language, on the same subject, which contains sounder theoretical views, and there is none which can be more practical than it has been to meet the demands of the engineer and the architect. From the theoretical expositions here given of the neutral line, the experiments to determine the strongest beam were devised and successfully carried out.

The result was the discovery of the celebrated "Hodgkinson's Beam," that is, the strongest beam which can be made from a given weight of material and a given length and depth of beam. George Stephenson, who was at this time chief engineer to the Manchester and Liverpool Railway Company, took great interest in these experiments,

and he was frequently present when they were made. Several pages are devoted again to the subject of the neutral line, indicating, from the manner of its discussion, that the subject was not at this time clearly fixed in the minds of the foremost investigators; and no one can read these pages, and the views of Prof. Barlow, without feeling convinced that the learned Professor has scarcely done full justice to Mr. Hodgkinson in reference to the fixing of the neutral line in the section of fracture. The statement of Prof. Barlow, in his Report to the British Association, and in his Essay 'on the Strength of Materials,' would lead to the conclusion that Mr. Hodgkinson had only rectified a small error into which he, Barlow, had inadvertently fallen. This is not a complete statement. Mr. Hodgkinson did much more than correct a slight error in an adopted theory; he showed the fallacy of the theory which, it appears, Prof. Barlow had obtained from M. Duleau, a distinguished French writer. There can be no doubt that Mr. Hodgkinson was the first to give the correct theory of fixing mathematically the position of the neutral line. Mr. Hodgkinson's paper was published in 1822, and we find, in 1824, Dr. Whewell, in his 'Mechanics,' stating, "I would gladly have given a section on the strength and fracture of beams, had there been any mode of considering the subject which combined simplicity with a correspondence to facts. The common theory, which supposes the material incapable of compression, is manifestly and completely false; and though Mr. Barlow's experiments and investigations give us much information, they do not appear to lead to any conclusions sufficiently general and simple to authorize us to present the subject as an elementary one." (See Preface, page xii, Whewell's 'Mechanics,' 1824.) It is obvious that the learned Professor had not seen Mr. Hodgkinson's paper at this time, or he would have given, without doubt, in this place the same chapter

which he published in his 'Analytical Statics' in 1833 *.

The first series of experiments in this paper show that, in cast iron, the extensions and compressions from equal forces are nearly equal. Tredgold asserted that the same force which destroyed the elasticity of a body by tension would destroy it by compression. The next two experiments disprove this assertion, and show that the resistance to compression in cast iron is greater than to extension. This discovery is important, and modified considerably the best-constructed cast-iron beams of this period. The succeeding experiments, which are many and carefully recorded, were devised for the purpose of extending the consequences of this practical discovery. And I shall here avail myself of the Rev. Canon Moseley's concise and able exposition of the experiments and reasonings of Mr. Hodgkinson by which he established the best form of cast-iron beam.

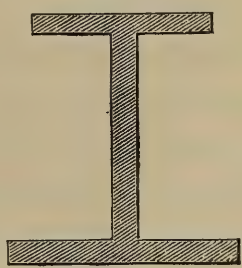
"Since the extension and the compression of the material are the greatest at those points which are most distant from the neutral axis of the section, it is evident that the material cannot be in the state bordering upon rupture at every point of the section at the same instant, unless all the material of the compressed side be collected at the same distance from the neutral axis, and likewise all the material of the extended side, or unless the material of the extended side and the material of the compressed side be respectively collected into two geometrical lines parallel to the neutral axis—a distribution manifestly impossible, since it would produce an entire separation of the two sides of the beam.

"The nearest practicable approach to this form of section

* I have Dr. Whewell's authority, in a letter which I received from him a few days ago, in stating that he had not seen Mr. Hodgkinson's paper when he wrote his 'Mechanics' of 1824.

is that represented in the accompanying figure, where the material is shown collected in two thin but wide flanges, but united by a narrow rib. That which constitutes the strength of the beam being the resistance of its material to compression on the one side of its neutral axis, and its resistance to extension on the other side, it is evidently a second condition of the strongest form of any given section, that when the beam is about to break across that section by extension on the one side, it may be about to break by compression on the other. So long, therefore, as the distribution of the material is not such as that the compressed and extended sides would yield together, the strongest form of section is not attained. Hence it is apparent that the strongest form of the section collects the greater quantity of the material on the compressed or the extended side of the beam, according as the resistance of the material to compression or to extension is the less. Where the material of the beam is cast iron, whose resistance to extension is greatly less than its resistance to compression, it is evident that the greater portion of the material must be collected on the extended side.

Fig. 3.



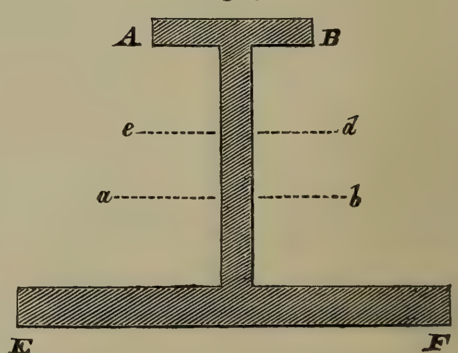
"Thus it follows, from the preceding condition and this, that the strongest form of section in a cast-iron beam is that by which the material is collected into two unequal flanges joined by a rib, the greater flange being on the extended side, and the proportion of this inequality of the flanges being just such as to make up for the inequality of the resistances of the material to rupture by extension and compression respectively. Mr. Hodgkinson, to whom this suggestion is due, has directed a series of experiments to the determination of that proportion of the flanges by which the strongest form of section is obtained.

"The details of these experiments are found in the following Table:—

No. of Experiment.	Ratio of the section of the flanges.	Area of whole section in square inches.	Strength per square inch of section.
1	1—1	2·82	2368
2	1—2	2·87	2567
3	1—4	3·02	2737
4	1—4·5	3·37	3183
5	1—5·5	5·0	3346
6	1—6·1	6·4	4075

"In the first five experiments, each beam broke by tearing asunder of the lower flange. The distribution by which both were about to yield together, that is, the strongest distribution, was not therefore up to that period reached. At length, however, in the last experiment, the beam yielded by the compression of the upper flange. In this experiment, therefore, the upper flange was the weakest; in the one before it, the lower flange was the weakest. For a form between the two, therefore, the flanges were of equal strength to resist extension and compression respectively, and this was the strongest form of section. In this strongest form, the lower flange had six times the material of the upper. It is represented in the accompanying figure. In the best form of cast-iron beam or girder used before these experiments, there was never attained a strength of more than 2885 lbs. per square inch of section. There was therefore by this form a gain of 1190 lbs. per square inch of the section, or of two-fifths the strength of the beam." (See Moseley's 'Engineering and Architecture,' art. 411.)

Fig. 4.



The Rev. Canon Moseley further observes on this point, "It is only in cast-iron beams that it is customary to seek an economy of the material in the strength of the section of the beam; the same principle of economy is surely, however, applicable to beams of wood."

This victory over the material foe is entirely Mr. Hodgkinson's own; and, using the language of the President of the British Association at Manchester, 1861, there is no one to divide the honour of this useful achievement with him. The Hodgkinson beam is really what its name would imply, as he originated the conception and pursued it with judgment and industry until the best form of beam was fully determined. This beam has been the pole-star for engineers and builders during the last twenty years—a period in which construction of all kinds has been in great demand, and in which the ingenuity and skill of the constructor has been confronted with many and formidable difficulties. Railways, ship-building, and public works of various kinds have opened out new channels for the application of cast and wrought iron; and when this material is placed in new and untried positions, it is no little point which is gained when its tensile and crushing strength is determined, and the best form investigated by which the safety of large structures is secured. This was the life-work of Prof. Hodgkinson.

It is a great thing, which no man of science lightly appreciates in these days of mental activity, for a man to point to a useful discovery and claim it as his own without a rival,—to say to himself (his own precious reward), "I drew it forth, from the dark chaos in which it had been entombed for ages, to the light of day, and now I leave it as a legacy to my countrymen, trusting that the chance of calamities such as that which happened at Hartley Colliery, where 200 men lost their lives by the breaking of a cast-iron beam, may be diminished, if not entirely obviated." In

this paper Mr. Hodgkinson acknowledges his deep obligations to the liberality of his friend Mr. Fairbairn, in procuring for him the beams whereon to experiment.

The contributions of Prof. Hodgkinson to the "Reports" and "Sections" of the British Association were numerous and important. In proof of this it is only necessary to refer to the opening address of the President, Professor Sedgwick, at the Meeting at Edinburgh in 1834:—"The Association may claim some credit for having brought into general notice the ingenious investigations of Mr. Hodgkinson of Manchester."

In the Report of 1833 there are two papers by Mr. Hodgkinson—

1st, "On the Effect of Impact of Beams."

2nd, "On the direct Strength of Cast-iron."

In the Report of the British Association of 1834, we find an extended inquiry into the collision of imperfectly elastic bodies. After alluding to Newton's labours, as recorded in the 'Principia,' Mr. Hodgkinson proceeds to describe the methods by which his experiments were made, and derives from them the following conclusions:—

1. All rigid bodies are possessed of some degree of elasticity, and among bodies of the same nature the hardest are generally the most elastic.

2. There are no perfectly hard inelastic bodies, as assumed by the early and some of the modern writers on mechanics.

3. The elasticity, as measured by the velocity of recoil divided by the velocity of impact, is a ratio which (though it decreases as the velocity increases) is nearly constant when the same rigid bodies are struck together with considerably different velocities.

4. The elasticity, as defined in (3), is the same whether the impinging bodies be great or small.

5. The elasticity is the same, whatever be the relative weights of the impinging bodies.

6. On impacts between bodies differing very much in hardness, the elasticity with which they separate is nearly that of the softer body.

7. In impacts between bodies whose hardness differs in any degree, the resulting elasticity is made up of the elasticities of both, each contributing a part of its own elasticity in proportion to its relative softness or compressibility.

The following rule, given by Mr. Hodgkinson, agrees remarkably well with the results of experiments :—

$$\left.\begin{array}{ll}\text{Let } \epsilon = & \text{the elasticity of A} \\ \epsilon' = & \text{do.} \qquad\qquad \text{B}\end{array}\right\} \text{as determined by A striking against A, \&c.}$$

$$\left.\begin{array}{ll} m = & \text{modulus of elasticity of A} \\ m' = & \text{do.} \qquad\qquad\qquad \text{B}\end{array}\right\} \text{as determined by extending the material in the ordinary way.}$$

Then the elasticity of A against B $=\dfrac{\epsilon m' + \epsilon' m}{m' + m}$.

This paper concludes with a table of elasticities of sixty various substances used in the construction of buildings, &c.

The Fifth Report of the British Association contains a paper on the "Impact of Beams."

The author has deduced from the experiments the following laws :—

1. If different bodies of equal weight, but differing considerably in hardness and elastic force, be made to strike horizontally with the same velocity against the middle of a heavy beam supported at its ends, all the bodies will recoil with velocities equal to one another.

2. If, as before, a beam be struck horizontally by bodies of the same weight, but different in hardness and elastic

force, the deflection of the beam will be the same, whichever body be used.

3. The quantity of recoil in a body, after striking against a beam as above, is nearly equal to what would arise from the full varying pressure of a perfectly elastic beam as it recovered its form after deflection.

4. The effects of bodies of different natures striking against a hard flexible beam seem to be independent of the elasticities of the bodies, and may be calculated, with trifling error, on a supposition that they are inelastic.

5. The power of a uniform beam to resist a blow given horizontally is the same in whatever part it is struck.

6. The power of a heavy uniform beam to resist a horizontal impact is to the power of a very light one as half the weight of the beam, added to the weight of the striking body, is to the weight of the striking body alone.

7. The power of a uniform beam to resist fracture from a light body falling upon it (the strength and flexibility of the beam being the same) is greater as its weight increases, and greatest when the weight of half the beam, added to that of the striking body, is nearly equal to one-third of the weight which would break the beam by pressure.

There can be but one opinion as to the importance of these deductions, direct from the voice of nature, made, as they were, at a time when such an appeal was by no means common.

There are several interesting problems on impact, of a high mathematical character, solved in this paper. In these inquiries Mr. Hodgkinson is very particular in acknowledging his many obligations to his friend Mr. Fairbairn, engineer, of Manchester, to whose labours and liberality practical science is deeply indebted.

We now pass on to notice his contributions to the Transactions of the Royal Society.

In the Philosophical Transactions for 1840 there is an

extensive inquiry, by Mr. Hodgkinson, "On the Strength of Pillars of Cast Iron and other Materials."

The object of this inquiry is to supply a desideratum in practical mechanics, which had been pointed out by Dr. Robison and Prof. Barlow. In order to accomplish this, it was necessary to institute a series of expensive experiments, more varied and extensive than any which had hitherto been made public. The subject was mentioned to Mr. Fairbairn, who at once, with his characteristic liberality, supplied his friend with ample means for investigating experimentally the strength of cast-iron pillars. For this paper the Council for the Royal Society awarded Mr. Hodgkinson the Royal Medal as a mark of their appreciation of his labours, the value and importance of which are confirmed by every engineer's pocket-book in Europe during a period of twenty years.

The inquiry is naturally divided into two parts, viz. Long Pillars and Short Pillars.

Long Pillars.

The first object was to supply the deficiencies of Euler's theory of the strength of pillars, if it should appear capable of being rendered practically useful, and if not, to endeavour to adapt the experiments so as to lead to useful results. For this purpose solid cast-iron pillars were broken, of various dimensions, from five feet to one inch in length, and from half an inch to three inches in diameter. In hollow pillars the length was increased to seven feet six inches, and the diameter to three inches and a half.

With pillars of cast iron, wrought iron, steel, and timber, whose length is upwards of 30 *times their diameter, the strength of those with flat ends is three times as great as those with rounded ends.*

Experiments were next made upon pillars with one end

flat and the other end rounded, and the result is summed up in the following interesting and important law:—

With pillars of the same diameter and length, both ends rounded, one end rounded and the other flat, and both ends flat, their strengths are as 1, 2, 3 *respectively.*

When the pillars were uniform, and the same shape at both ends, the fracture took place in the middle. This was not the case when one end was flat and the other rounded, as the fracture then took place at about one-third of the length from the rounded end. Hence in these pillars the metal may be economized by increasing the thickness in the point of fracture.

It follows from Euler's theory, that the strength of pillars to bear *incipient flexure* is directly as the fourth power of the diameter, and inversely as the square of the length.

This incipient flexure was sought for by Mr. Hodgkinson without success, and he states his conviction that flexure commences with very small weights, such as could be of little use to load pillars with in practice. Although Mr. Hodgkinson was unable to find the point to which Euler's computations refer, still he has shown that Euler's formula is not widely from the truth when applied to the breaking-point of the pillar. From a great number of experiments, Mr. Hodgkinson deduced the following formula for pillars with rounded ends:—

D = diameter of pillar in inches.
L = length of pillar in feet.
W = breaking-weight in tons.

Then, $W = 14{\cdot}9 \dfrac{D^{3{\cdot}76}}{L^{1{\cdot}7}}$.

The above rule applies to pillars the length of which is fifteen times the diameter and upwards. Perhaps not quite so low as fifteen times the diameter in large pillars, as there

is a reduction of the strength of such pillars, owing to the softness of the metal in large castings. This remark is significant, and gave rise to many interesting experiments at Portsmouth Dockyard by the Royal Commissioners, conducted by Col. Sir Henry James.

When the pillars are flat at the ends, the formula becomes

$$W = 44 \cdot 16 \frac{D^{3 \cdot 55}}{L^{1 \cdot 7}}.$$

This rule applies to pillars whose lengths vary from 30 to 121 times the diameter.

Short Pillars.

In order to estimate the breaking-strength of short pillars, Mr. Hodgkinson considered the strength of the pillar to be made up of two functions.

1st. To support the weight.

2nd. To resist flexure.

When the breaking-weight is small, as in long pillars with small diameters, then the strength of the pillar will be employed in resisting flexure. When the breaking-weight is one-half the pressure required to crush the pillar, one-half of the strength may be considered available to resist flexure, and the other half to resist crushing. And when the breaking-weight is so great as in the case of short pillars, it may be considered that no part of the strength of the pillar is applied to resist flexure. These two effects may be separated in all pillars, by dividing the pillar into two portions, one of which would support the weight without flexure, and the other would support the flexure without crushing, to the extent indicated by the preceding formulæ.

Let c = the force which would crush the pillar without flexure.

Let P = the utmost pressure the pillar would bear without being weakened by crushing.

b = breaking-weight as calculated by the preceding formulæ.

y = the actual breaking-weight of short pillars.

$$\therefore \frac{y}{b} = \frac{1}{\frac{b}{c} + \frac{3}{4}} \text{ where } P = \frac{c}{4}.$$

The value of c is obtained from the formula

$$c = (\text{area of section}) \times 109{,}801 \text{ lbs.}$$

The reasoning by which the above formulæ are established is well deserving of attention, and shows that the author was a worthy successor of Euler, Lagrange, and Poisson in this important branch of practical science.

Hollow Pillars of Cast Iron.

Mr. Hodgkinson has shown that solid pillars with rounded ends and enlarged in the middle are stronger than uniform pillars of the same length and weight. This is proved to be the case in hollow pillars. The formulæ for the breaking-weight of hollow pillars, as derived from experiment, are as follows :—

w = breaking-weight in lbs.
D = external diameter in inches.
d = internal diameter in inches.
L = length in feet.

For pillars with rounded ends,

$$w = 29074 \frac{D^{3 \cdot 76} - d^{3 \cdot 76}}{L^{1 \cdot 7}}.$$

For pillars with flat ends,

$$w = 99318 \frac{D^{3 \cdot 55} - d^{3 \cdot 55}}{L^{1 \cdot 7}}.$$

The strength of short hollow pillars must be calculated

in the same manner as the strength of short solid pillars. These formulæ, derived from experiments made with great judgment and care, embody our present knowledge and practice of cast-iron pillars for bearing-purposes.

"The Power of Cast-iron Pillars to resist long-continued pressure."

Mr. Hodgkinson has recorded in this paper several very interesting experiments on this subject. Two beams, rounded at the ends, 6 feet long and 1 inch diameter, were cast of Low Moor iron, No. 3. The first bore a weight of 1456 lbs. during a period of from five to six months, and then broke. The second broke with 1500 lbs. laid on immediately. From this experiment Mr. Hodgkinson inferred that time has but little, if any, influence on the strength of cast iron.

This inference seems, at least to me, to be theoretically correct. If the weight laid on the beam and its molecular forces be statically equal, the forces will remain in this state of equilibrium until the molecular forces are weakened by the influence of unequal temperature or other causes. Our knowledge, however, of this practical subject is indeed very limited. The inquiry would amply repay any one who has the ability, opportunity, and means to pursue it. Mr. Hawkshaw has made some admirable remarks on this subject, in his evidence before the Royal Commissioners, in 1847 (see Report, p. 296).

The opinion of experienced engineers appears to be, that vibrations produced by continual impact and change of temperature affect the strength of iron to a greater extent than a continued strain, which preserves the molecules of the iron in the same fixed position. Mr. Rastrick, in his evidence before the Commissioners, gives the result of an experiment made by a friend, bearing on this question, at Pontypool Iron Works. He hung a bar of iron, an inch square, up by one end perpendicularly, and contrived a small hammer to be continually hammering it; after a

period of more than twelve months, the bar of wrought iron dropped in two.

That the internal structure of iron becomes changed by continued vibrations is commonly believed by engineers of experience; but in what way this change is produced, both in speciality and magnitude, does not appear to be very definite. One thing, however, seems clear, viz. that wrought iron is more affected by vibrations than cast. The evidence given before the Commissioners on this important question is very striking, and contains all the practical information which has been recorded or known on the subject. Mr. Fairbairn states "that if you take *any material* whatever, and destroy its original form, and repeat the changes, it is only a question of time how long it will be before it breaks."

According to my view, this statement, from an engineer of so great experience, should convince those whose duty leads them to the application of iron, timber, and stones to the erection of structures the first characteristic of which is stability, of the existence and destructive nature of vibrations. Notwithstanding these views on the effect of continued vibrations, there are not wanting engineers of great eminence who think the subject of but little practical importance, however interesting it may be in a scientific and philosophical sense.

The late Robert Stephenson refers to the beam of a Cornish engine, and states that it receives a shock eight or ten times a minute, equal to about 55 tons, during a period of 20 years, without the slightest perceptible change in its structure and strength.

The connecting-rod of a locomotive engine is another illustration in point: "one I know," says Mr. Stephenson, "which has run 50,000 miles, and received a violent jar eight times per second, or 25,000,000 vibrations, and yet there is not the slightest appearance of change in the strength of the connecting-rod."

The same distinguished engineer says, with respect to the question of the effect of vibrations on materials, "as to the change being produced in wrought iron, which is a very popular and almost universal theory now, I have not known one single instance in which I have traced it to its origin, where the reasoning is not deficient in some important link." On the whole, Mr. Stephenson attaches but little importance to the question of vibration in a practical sense.

Mr. Brunel, in answer to the question whether the internal structure of an iron beam becomes altered by a succession of slight blows at a low temperature, as in rails long used, railway-axles, or springs of carriages, says, "I have turned my attention a good deal to this inquiry, and I have long acted on the assumption that iron is so changed; but I must confess that I have doubts as to the fact. And I believe that if the subject were thoroughly examined, it would be found that the different appearances shown by iron when broken arise from the combinations of the causes producing fracture as often as from any change in the texture of the material itself. This opinion was strengthened by various specimens of irons broken, some with a fibrous fracture by means of a slow heavy blow, and some with a crystalline fracture by means of a sharp, short blow. Mr. May refers to the case of a cast-iron beam of a steam-engine, vibrating hundreds of thousands of times per annum, being as good at the end of 20 or 30 years as when first put up. In this case, though the strain has been in opposite directions, and constantly varying, still the vibrations have not weakened the beam. On the other hand, he says, I have seen a cast-iron gun absolutely broken across by many years' dropping pig-iron upon it."

In order to facilitate the calculation of the strength of short pillars, Mr. Hodgkinson has given the crushing-strength of a great variety of timbers used in practice.

The above is but a hasty and imperfect glance at this important paper, which appeared at the time when the railway system was developing itself by means of the application of cast- and wrought-iron pillars to the construction of bridges, &c. No engineer who has in future to deal with this subject must omit the reading of this paper.

In the Philosophical Transactions for 1857, there is another paper by Mr. Hodgkinson on the strength of pillars. The object here is to confirm the conclusions of the first paper by means of larger experiments, made by an apparatus three times as great as the apparatus used on the former occasion. Having been unsuccessful in finding the weight producing incipient flexure, Mr. Hodgkinson devoted his attention to finding the breaking-weight, the deflection, and decrement of length produced by the weight laid on the pillars. The pillars with both ends rounded broke in one place, in the middle; but the pillars with both ends flat broke in three places, the middle, and at each end. When one end was flat and the other rounded, it broke at one-third the distance from the rounded end.

The formulæ in the former paper are here slightly corrected, as being more in accordance with the results of larger experiments.

Thus, in pillars whose ends are flat and well bedded, the formula becomes

$$w = 42{\cdot}347 \frac{D^{3{\cdot}5} - d^{3{\cdot}5}}{L^{1{\cdot}63}},$$

instead of

$$w = 46{\cdot}65 \frac{D^{3{\cdot}55} - d^{3{\cdot}55}}{L^{1{\cdot}7}},$$

as given in the first paper.

It is a matter of observation long recorded, both by Mr. Hodgkinson and other experimentalists, that the metal in large castings is not uniform in density, the density dimin-

ishing from the outside of the casting to the centre. Hence it was justly inferred that the crushing, tensile, and transverse strength of large castings would vary, being the greatest towards the outside, and less towards the centre. In cast-iron pillars of $2\frac{1}{2}$ inches diameter, the crushing force varied from 39 tons per square inch outside to $33\frac{1}{2}$ tons per square inch centre. Mr. Hodgkinson discovered that the difference in the strength between the outside and centre of large castings is much less than in small ones. Col. Sir Henry James found that the central part of bars of iron planed was much weaker to bear transverse strain than bars of the same size. By planing out $\frac{3}{4}$-inch bars from the centre of 2-inch square and 3-inch square bars, the central portion was little more than half the strength of that from an inch bar.

The fall of the railway bridge over the River Dee at Chester, when several lives were lost, led Mr. Hodgkinson to investigate the position of the tension-rods, which were intended as auxiliary supports to the structure. The particulars of this inquiry have escaped my memory; but I well remember that Mr. Hodgkinson showed, on the clearest geometrical evidence, that the position of the tension-rods was not only no additional support to the stability of the bridge, but positively aided its downfall. This circumstance induced Mr. Stephenson to reconsider the construction of the bridge, and devise a new arrangement for these auxiliary supports. It was at this time, and in consequence of the accident above alluded to, that Mr. Robert Stephenson made the personal acquaintance of Mr. Hodgkinson, the friend of his father, the man to whom he had steadily looked as his authority and guide in the application of iron to railway purposes. When, therefore, Mr. Stephenson was engaged in the novel construction of the Conway and Britannia Tubular Bridges, he requested the assistance of his friend Mr. Hodgkinson in fixing the best form and

dimensions of tubes. The experiments which were devised and carried out by Mr. Hodgkinson with a view to answer the above questions are recorded in the Report of the Royal Commissioners appointed to inquire into the application of iron to railway structures.

Mr. Hodgkinson, by these experiments, sought—

1. To ascertain how far the strain upon a square inch at the top and bottom of the tube would be affected by changing the thickness of the metal, the other dimensions being the same.

2. To obtain the strength of similar tubes.

3. To find the strength of tubes of various forms of section in the middle, and to furnish means of judging of the proper proportions of the metal in the bottom, top, and sides of the tube.

4. To ascertain the relative strength of uniform tubes to bear a weight in all parts of their length; and whether tubes, tapering in thickness from the middle towards the ends, according to theory, would be equally strong in every part.

5. To obtain the resistance of the tubes, previously tried vertically, to bear a side pressure, with an intention to ascertain the effect of the wind upon a tube.

6. To ascertain the strength of small tubes of different forms of section to resist best a force of compression applied in the direction of their length.

7. To ascertain the resistance of wrought-iron plates to a crushing force in the direction of their length.

8. To determine the strength of tubes to sustain impact, with reference to riveting.

9. To determine, by bodies let fall upon tubes, the probable effect, if any, of trains rushing rapidly upon tubular bridges, to produce resilience, or springing up at the ends.

10. To determine the transverse strength of tubes stiffened in the top with cast iron, joined with wrought

iron, to increase the resistance of the top to a crushing force.

These are important practical problems; and when the issue is considered, viz. the continued stability of the Conway and Britannia Tubular Bridges, they required for their solution great skill in the subtilties and artifices of mathematical and experimental science. The answers which Mr. Hodgkinson obtained to the above problems were deemed by Mr. Stephenson to be so satisfactory as to enable him with confidence to build the Tubular Bridges.

A concise but clear exposition of these answers is given by Mr. E. Clark before the Commissioners appointed to inquire into the application of iron to railway purposes (see Report, page 359).

It was impossible that such assistance in the execution of a novel design could be lightly esteemed or inadequately appreciated by the great engineer. Hence, in the history of these tubular bridges, where Mr. Stephenson is anxious to record the merits of his assistants, he frankly acknowledges his deep obligations to the mathematical philosopher "*for devising and carrying out a series of experiments which terminated in establishing the laws that regulate the strength of tubular structures, in a manner so satisfactory that I was enabled to proceed with more confidence than I otherwise should have done*" (see vol. i. p. 35, of the 'Britannia and Conway Tubular Bridges,' by E. Clark, Esq.).

This declaration of Mr. Stephenson completely disarms all future praise or detraction with respect to the part which Mr. Hodgkinson took in the execution of the tubular bridges. It places him before the public in his right position as a most important contributor to the success of an enterprise which will represent the engineering skill of the present time, and will be the admiration of future ages. E. Clark, Esq., who superintended the building of the

tubular bridges, speaks in the highest terms of the importance of Mr. Hodgkinson's labours in fixing the proper dimensions of the bridges.

We are indebted to him also for nearly the whole of the mathematical calculations in reducing the experiments which were made into a form fit for application to a large structure. But we are also indebted to Mr. Fairbairn for a great portion of the practical construction of the bridges.

The answers given by Mr. Hodgkinson to his inquiries, and which rendered such signal service to the engineer in the execution of his novel design, are as follows :—

1. The value of (f) the strain upon a square inch at the top or bottom of the tube is constant in material of the same nature, while it varies from 19, 14, to $7\frac{3}{4}$ tons when the thickness of metal varies from ·525, ·272, to ·124 of an inch. The determination of (f) is the chief obstacle to obtaining a formula for the computation of the strength of tubes of every form.

The strength of the Conway tube was calculated to bear 1084 tons when the value of (f) was taken at 8 tons, and the deflection about $15\frac{1}{2}$ inches in the middle.

2. The strength of similar tubes was somewhat lower than the square of their linear dimensions, being about 1·9 power instead of the square.

3. The tubes may be reduced in strength and thickness towards the ends, corresponding to the ratio indicated by theory, viz. that the strain at any point of the tube is proportional to the rectangle of the two parts into which that point divides the length of the tube.

4. The power of the tube to resist a vertical strain is to its power to resist a strain on its side, as from the wind, as 26 to 15 nearly.

5. The resistance of tubes to crushing follows the law of cast-iron pillars when the crushing force is not more than

8 tons per square inch. It appears, however, that cast iron was decreased in length double what wrought iron was by the same weight; but the wrought iron sunk to any degree with a weight of 12 tons per square inch, while cast iron required double the weight to produce the same effect.

6. The power of plates to resist buckling varies nearly as the cube of the thickness. Mr. Clark refers to this property as being most useful in the construction of the tubular bridge.

7. The tube bent by pressure had borne a deflection of 5 inches without serious injury; but its riveting was destroyed by repeated impacts deflecting it through less than one inch.

8. Resilience is perceptible, but very small.

9. The introduction of cast iron on the top of the tube would be attended with advantage in resisting the force of compression. Practical objections, however, of a serious nature prevented Mr. Stephenson from availing himself of the power of cast iron to resist compression. He thought it advisable to increase the thickness of wrought iron to resist compression, rather than use a combination of wrought with cast iron. It may be stated that Mr. Stephenson has used cast iron, for the purpose recommended by Mr. Hodgkinson, with success in tubes of smaller dimensions than the Conway tubes.

In 1847 Mr. Hodgkinson was appointed one of the Commissioners to inquire into the application of iron to railway structures; and during the space of two years the whole of his time and abilities were devoted to the subjects of this inquiry. The exertions, both physical and mental, which he made at this period for the advancement of engineering science were so great as materially to affect his health and prostrate his powers. Immediately after the publication of the Commissioners' Report in 1849, he

sought the restoration of his exhausted faculties by a tour on the continent of Europe.

His labours for this Commission are published in the Report, and comprise 114 closely printed pages. The high importance of these labours may be, to some extent, inferred from the circumstance of the Commissioners pointing them out for special notice. "Although we are aware that to point out the labours of individual members of the Commission would be impossible, and that it may appear invidious to single out one for praise, we cannot resist the expression of our thanks to Mr. Hodgkinson for the zeal and intelligence with which he has carried out the remarkable series of experiments which are detailed in the Appendix A to this Report, and which constitute a large proportion of those which have been already described" (see the Commissioners' Report, page 15). Such, then, was the estimate of the labours of Mr. Hodgkinson by Lord Wrottesley, Prof. Willis, Col. James, Mr. Rennie, and Mr. Cubitt; and it has been amply confirmed by the engineering experience of the last thirteen years. The objects for which Mr. Hodgkinson sought in this inquiry were—

1. The determination of the longitudinal extensions and compressions of long bars of cast and wrought iron by weights varied by equal increments, up to that producing fracture.

2. The establishment of general formulæ connecting the longitudinal *extensions*, and *compressions*, and *sets* of cast iron with the forces producing them.

3. To determine the deflection of horizontal bars produced by various transverse pressures, and to compare the effects with those produced by impacts.

4. To determine general formulæ connecting the transverse pressure, the deflection, and set remaining after the pressure was removed.

If ϵ=elongation of a bar of cast iron one inch square and (l) inches long by a weight w,

$$\text{then } w=13934040\frac{\epsilon}{l}-2907432000\frac{\epsilon^2}{l^2}.$$

If d=compression of a bar of cast iron one inch square and (l) inches long by a weight w,

$$\text{then } w=12931560\frac{d}{l}-522979200\frac{d^2}{l^2}.$$

These formulæ were derived from the mean results of four different kinds of cast iron.

The mean tensile strength was found to be 15711 lbs. per square inch, and the ultimate extension was 1-600th of the length of the bar.

With respect to wrought iron, the extensions and compressions were found to be nearly proportional to the pressures producing them.

The extension is proportional to the pressure up to about 12 tons per square inch, after this the pressure is not proportional to the extension. The weight necessary to elongate a bar of wrought iron to double its length is 27,691,200 lbs., which is usually called the modulus of elasticity. One striking and important fact was elicited by these experimental researches, viz. cast-iron bars are decreased in length double as much as wrought-iron bars by the same pressure; but wrought-iron bars sink to any degree with little more than 12 tons pressure per square inch of section, while cast iron-bars require three times the pressure to produce the same effect. It appears also that the tensile force of cast iron depends but little upon the form of the section, except so far as the form contributes to the better consolidation of the casting when in a fluid state.

The above results were obtained for the Commissioners by the individual labours of Mr. Hodgkinson himself, who alone is responsible for their accuracy, usefulness, and general adaptation to promote the ends of physical and

engineering science; but there were other important results obtained by other Members of the Commission, to which it may not be deemed out of place to refer.

The experiments at Portsmouth Dockyard, conducted by Colonel Sir Henry James, and the discussion of the results by Prof. Willis and Prof. Stokes, were also the work of the Commissioners. And it would be no easy task to overestimate the value of these labours, both on account of the novel nature of the experiments and the mathematical deductions to which they conducted when placed in the hands of Prof. Stokes.

Col. Sir Henry James and Capt. Galton subjected cast-iron bars, placed between fixed supports, to 100,000 successive deflections, at the rate of four per minute, by means of a cam. When the deflections were one-third of the ultimate deflection, the bars were not weakened; when, however, the deflections were one-half of the ultimate deflection, the bars were broken with less than 900 depressions.

Prof. Hodgkinson subjected cast-iron bars, firmly fixed between supports, to 4000 continued impacts. When the blow was such as to deflect the bars one-third of their ultimate deflection, they resisted the concussion of 4000 impacts without injury; but when the blow was such as to deflect the bars one-half of their ultimate deflection, no bar could resist 4000 depressions. These results strikingly confirm each other.

Col. James and Capt. Galton caused a weight, equal to one-half the breaking-weight of the cast-iron bar, to be drawn backwards and forwards from one end of the bar to the other. The bar was not weakened by 96,000 transits of the weight. No perceptible effect was produced in wrought-iron bars by 10,000 successive deflections, each of which was equal to that produced by half the breaking-weight.

Prof. Hodgkinson notices the following results, which he obtained from his experiments on the impact of cast-iron bars :—

All cast-iron bars of the same sectional area require the same blow to break them in the middle.

The deflections of wrought-iron bars produced by the striking ball were proportional to the velocity of impact; but in cast-iron bars the deflections were greater than the proportion to the velocity of impact.

The most striking and novel experiments, however, were those made by Col. Sir Henry James and Capt. Galton at Portsmouth Dockyard. These gentlemen constructed a large apparatus by which weights could be made to move over cast-iron beams placed horizontally between fixed supports, with velocities varying from 0 to 30 miles per hour. These experiments developed the singular fact, at variance with the impressions of the most eminent engineers, that a train passing over a bridge at a given speed will produce a greater deflection than that produced by the train being placed upon the bridge in a state of repose. This important fact was confirmed in all its entirety by the larger experiments made by the Commissioners on the Ewell Bridge, on the Epsom line, and the Godstone Bridge, on the South-eastern line.

Col. James found that when a carriage was loaded with 1120 lbs. and placed at rest upon a cast-iron bar, it produced a deflection of six-tenths of an inch; when, however, the carriage moved over the bar at the rate of ten miles per hour, the deflection was increased to eight-tenths of an inch; when the speed of the carriage was increased to thirty miles per hour, the deflection was increased to one inch and a half, which is more than double the statical deflection. It follows from this that a much less weight will break a bar of cast iron when it moves over it at a great speed, than if it be placed at rest upon the bar. The

bars, when broken by a load passing over them, were fractured at points beyond their centres, often into four or five pieces, indicating the unusual strains to which they had been subject. From these unexpected results there is no appeal, however much they may be at variance with the impressions of the most gifted engineers. It now remains to connect these results with well-established mechanical laws, a problem of great difficulty, the solution of which has been accomplished by the labours of Prof. Willis and Prof. Stokes (see 'Preliminary Essay on the Effects produced by causing Weights to travel over Elastic Bars,' by the Rev. Robert Willis, F.R.S., &c.).

By neglecting the inertia of the bar, as being small in relation to the moving weight, Prof. Stokes has shown that

$$D = S + \tfrac{1}{2}\left(\frac{VS}{l}\right)^2.$$

D = central dynamical deflection of the bar, produced by the weight moving at the velocity V.

S = central statical deflection produced by the same weight.

l = the length of the bar in feet.

Hence the dynamical deflection is double of the statical, when the velocity of the moving weight is $\sqrt{2}$ times the length of the bar between the supports.

These results were not readily accepted by practical men, as they had been accustomed to connect high velocities of the train with small deflections of the bridge over which it passed.

The late Robert Stephenson, in his evidence before the Commissioners, states that he had seen the deflections less as the train passed over than when it was in repose. From the observations which he had made, he felt quite satisfied upon the point, that no revision of the practical rules re-

specting the deflection of the bridges was necessary. "You will sometimes find," he adds, "an exceptional case occurs, if the engine happen to jump on the springs, which may, of course, accidentally occur; but if it be a mere question of velocity, I do not think it increases the strain upon the girder. There may be a lateral strain backwards and forwards when the whole train comes into play and causes a jerk."

Mr. Locke, after making many experiments with locomotives passing over bridges, arrives at the conclusion that there is but little difference in the deflection between high velocities and low. "If there be," he remarks, "three or four bad rails or joints upon the top of a bridge, there is far more effect produced upon the bridge. A bad joint is more serious than 10 or 12 miles' increase or diminution of velocity."

Mr. Hawkshaw's opinion is, that there would be a greater deflection in a bridge by running a weight over it, than by allowing the same weight to rest upon it—because there is always an irregularity in the surface of the rails, and the force of impact is thereby brought into activity. W. H. Barlow stood under a wooden viaduct while a heavy goods train passed over it. There was a slight deflection produced by the heavy train; but the express, with a much lighter engine, and moving at a greater speed, produced a much worse effect. It seemed to produce a wave through the bridge, as it ought to do from the ordinary principles of dynamics. This load was passing over the bridge in a very few seconds, and therefore the total deflection is performed by the weight in a few seconds; and it therefore becomes a kind of blow—the descent of a heavy weight—and the bridge has not time to accommodate itself to the deflections required of it. These deflections are propagated throughout the structure, and may prove exceedingly dangerous and disagreeable.

Mr. Rastrick always considered that when a weight passed rapidly over a structure, there would be less deflection than if it were stationary. He takes the example, for comparison, of a man skating upon ice, and states that if he remain stationary for a length of time, he would soon go through the ice; but he may skate over it without any danger of going through, because the ice has no time to break.

Mr. Brunel's impression was that where the rails are perfect the deflection is, as it ought to be, less with a weight passing rapidly over it than when it rests upon it; "but the experiment is so difficult to make, from the number of interfering causes, that perhaps my impression is still only prejudice rather than positive information."

Mr. Cubitt, engineer of the Great Northern Railway, could perceive no difference in the deflection of a large girder between the weight being stationary upon it and passing over it at a great speed. The experiment was made upon a girder 47 feet span, and a heavy locomotive engine, the deflection being a tenth of an inch.

The opinion of Mr. Charles Fox, engineer, is very decided on this point. He states positively that, if the rails have been carefully laid over the portion of the line resting upon the bridge, less deflection is caused in the girder by a load passing at a high speed than at a low one, and that there is less deflection with any rate of speed than when the weight is stationary. "I imagine this arises, in a great measure, from the short time there is to overcome the inertia of the mass; of course the higher the velocity the less time is expended in the train passing over the bridge."

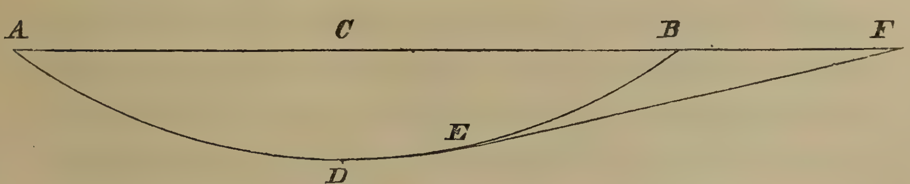
Mr. Glynn, of Butterly Iron Works, Derbyshire, thinks that, if the strength of the beam were not great in proportion to the stress it had to sustain, the weight, being stationary upon it, would tend to deflect it permanently more than a weight passing rapidly over it. "This opinion is not

formed from experience; experiments on the subject would be very desirable."

This is the testimony, conflicting as it is, of the highest authorities in the engineering profession respecting a most important part of their practice, viz. the permanent stability of structures over which thousands of people are being continually conveyed with rapid velocities.

Perhaps the simplest method to gain the conviction that the dynamical deflection of a structure is different from its statical deflection is to place a weight, capable of motion, and producing a sensible deflection, on the middle of a horizontal flexible beam, between fixed supports. Let us now inquire what effect is produced by moving the weight to a point very near to its original position. It is evident that the weight, being at the lowest point of the beam, cannot move from this position without the application of a force. The effect of this force upon the moving weight and flexible beam will, of course, depend upon its magnitude and direction. If the direction of the force be vertical, whatever may be its magnitude, it will not produce any horizontal motion in the moving weight. If the direction of the force, however, is not vertical, the case is very different.

The moveable weight, abandoned to the influence of gravity and the reaction of the beam, will have a complex, vertical and horizontal motion; while the flexible beam will be, from the same cause, put into a state of periodical oscillations, the number and amplitude of which will de-



pend upon the moving forces and inertia of the beam. Let

A B be the fixed supports of the beam, A D B its position after the weight has been placed upon it. If the point E in the beam is sufficiently near to D, then the line D E may be considered straight; produce D E to meet the horizontal line A B in F, and put the angle D F B $=\theta$. Let the force H, applied to move the weight from D to E upon the inclined plane D E, be in the direction of D E. It is evident that the force H can be decomposed into two, viz., H sin θ acting vertically upwards, and H cos θ acting horizontally from D to E. If we examine the effect of these two decomposed forces, it will be found that the force H cos θ, which is nearly equal to H, since the angle θ is small, will produce an elongation in the beam A D, and a compression in the beam D B. When the elongation of A D is greater than the compression of D B, the beam between the supports is increased in length, hence the middle point D, where the weight is placed, is moved vertically as well as horizontally into another position. From this force alone the beam would become a wreck if the force H, or the velocity with which the weight is moved from D to E, was sufficiently large; but, to prevent this catastrophe, the vertical component force H sin θ diminishes the reaction of the weight and beam. The vertical force of the weight, instead of being the weight alone, is now diminished by H sin θ, and is become W $-$ H sin θ, where W is the weight of the moveable body. The effect then of the vertical component is directly opposite to that of the horizontal component; and it is evident that under certain conditions either one or the other of these two forces may prevail. Hence the indications of theory are in harmony with the observations of engineers, and fully justify the conflicting evidence which they have given on the subject. Sometimes the conditions of the moving weight and the beam are such as to produce a statical deflection greater than the dynamical, and sometimes the conditions produce

a dynamical deflection greater than the statical. The computation of the effects of these component forces is attended with great difficulty, as they bring into full activity the elastic forces of the beam and its inertia. The solution, however, of this intricate problem, under certain restrictions, viz. when the weight of the beam is small compared with the moving weight, and the deflection small compared with the length of the beam, has been given by Prof. Stokes. See 'Transactions of the Cambridge Philosophical Society,' vol. viii. p. 707.

The same distinguished analyst has given another solution to this problem when the mass of the moving weight is neglected, or the effect of the weight reduced to a travelling pressure. The exact solution of the problem lies between these extreme cases, and is therefore circumscribed, by the labours of Prof. Stokes, in such a manner that it can be approached to any degree of proximity required. The general dynamical equations from which the dynamical deflection is computed, are so complex that a complete solution of the problem, as exhibited in practice by the moving weight being sustained by the beam in two points, is not likely soon to be furnished. Still, what has been accomplished by Professors Willis and Stokes is sufficient to show to practical engineers, that the startling results of Sir Henry James and Capt. Galton, as obtained at Portsmouth, and confirmed on the Ewell and Godstone Bridges, are indicated by dynamical laws, the truth of which cannot be controverted. If this be true—and there can be little doubt of it—no engineer will be justified in neglecting a just estimate of its effects on the stability of structures on the safety of which human life depends. The Commissioners appointed to inquire into the application of iron to railway structures, have rendered essential service to the public by the discovery and experimental development of the difference between statical and dyna-

mical deflection in iron girders. It is true they have not exhausted the subject, nor divested it wholly of its perplexity; but they have gained a positive and useful result, by showing to practical engineers the falsity of their position when they affirm that dynamical deflection is always less than the statical. I may state in conclusion, that Prof. Willis, by a train of reasoning which depends on the assumption of each particle of the beam moving into its position, forming the trajectory, at the same instant of time, has shown that the inertia of the beam is the same as it would be by placing half its weight at the centre.

This result is derived from a principle which is purely hypothetical, and the correct determination of which is the chief difficulty in the mathematical discussion of the problem. In the Appendix B to the Commissioners' Report, Prof. Willis has given the following dynamical equation, from which the trajectory of the curve described by the moving load may be computed:—

$$\frac{d^2y}{dx^2}=\frac{g}{V^2}-\frac{ga^4}{V^2S}\,\frac{y}{(2ax-x^2)^2}:$$

y and x are the rectangular coordinates of the moving weight, the origin being at the extremity of the beam; y is vertical, and x horizontal.

V = the velocity of the moving weight.
$2a$ = the length of the beam.
g = the force of gravity.
S = the central statical deflection.

This equation, and the reasoning by which it is established, accidentally fell into my hands during the time the Commissioners were considering it; and in a letter to the Secretary, Capt. Galton, I pointed out the hypothetical principle on which the equation is founded. This principle is, that the reaction between the moving weight and the

beam is equal to the weight which would be necessary to deflect the beam, when placed on it at rest, as much as the travelling load deflects it. This position is purely hypothetical, which may or may not give results approximating to the truth, according to the dimension of the quantities which constitute the fixed data of the problem. It is not improbable that this reaction, the amount and direction of which influence the motion of the moving weight over the beam, is continually vibrating between a maximum amount and zero, and that many times during the passage of the weight over the beam the reaction may be nothing, and therefore the moving load be abandoned to the influence of its own gravity only. However this may be, it is certain that its amount is never accurately measured by a formula which produces an accelerating force of

$$\frac{ga^4}{S} \frac{y}{(2ax-x^2)^2},$$

as given by Prof. Willis.

This subject has received considerable attention from Mr. H. Cox, in a paper entitled "Dynamical Deflection and Strain of Girders," which is printed in the 'Civil Engineer and Architect's Journal' for September 1848. It appears that Mr. Cox has established, from the principle of *vis viva*, that the moving body cannot in any case produce a deflection greater than double the central deflection, the elasticity of the girders being supposed perfect. Prof. Stokes, however, has shown that this conclusion of Mr. Cox is not true—that, among the sources of labouring force which can be employed in deflecting the girders, Mr. Cox has omitted to consider the *vis viva* arising from the horizontal motion of the body, and therefore has been led to an inference which is not correct. The recorded experimental facts connected with the dynamical deflection

of bridges and bars of iron are given in the Report of the Commissioners as follows :—

Ewell Bridge.

The span is 48 feet; the statical deflection, produced by the engine and tender 39 tons, and weight of half bridge 30 tons, was only ·215 inch. This deflection was increased to ·245 with a speed of 37 miles per hour. A speed of 51 miles per hour produced a deflection of ·235.

$$\frac{\text{Greatest dynamical deflection}}{\text{Statical deflection}} = 1 \cdot 14.$$

Godstone Bridge.

The span is 30 feet, the weight of engine and tender 33 tons, and weight of half bridge 25 tons; the statical deflection was ·19 inch. This was increased to ·25 by a speed of 49 miles per hour.

$$\frac{\text{The dynamical deflection}}{\text{Statical deflection}} = 1 \cdot 315,$$

showing an increase of nearly one-third.

A pair of steel bars, 2 feet 3 inches by 2 inches broad and $\frac{1}{4}$ inch deep, gave the following results :—

Velocity in feet per second		15	14	29	34	44
Central deflection in inches	·70	1·02	1·32	1·45	1·30	1·03

A bar of wrought iron 9 feet long, 1 inch broad, and 3 inches deep, with a load of 1778 lbs., gave the following results :—

Velocity in feet per second		15	29	36	43
Central deflection in inches.........	·29	·38	·50	·62	46

In the Commissioners' Report, Mr. Hodgkinson has given the results of a variety of experiments on the trans-

verse strength of cast, mixture of cast and wrought, and wrought iron. The experiments were made with great care; and every source of error that could be was eliminated, notwithstanding the trouble and expense which such a procedure necessitated. Still there was a great difficulty, which was always felt by Mr. Hodgkinson, and which occupied, at various times, much of his attention, viz. to connect the breaking weight of the beam with its deflection in such a manner as to indicate true practical results. For this purpose he entered into a very general theoretical investigation on the transverse flexure of beams, which is given in the 2nd volume of Tredgold 'On the Strength of Cast Iron'; but, in order to make the results of this very general investigation practical, he is compelled to assume, 1st, that the forces of extension and compression are proportional to the extensions and compressions; 2nd, that the force of extension is equal to the force of compression; 3rd, the reaction at the points of support is always vertical. It is not surprising, then, that a formula, based upon so many assumptions, should fail to represent correctly the relation between the breaking weight and the dimensions of the beam; this is exactly what has taken place.

The discordance here alluded to has arrested the attention of W. H. Barlow, Esq., C.E., F.R.S.; and the results of his investigations are given in two very interesting memoirs, printed in the 'Transactions of the Royal Society' for 1855-1857. It would be great presumption on my part to enter into any profound criticism on the mode of procedure and results of these memoirs, revised as they have been by Professor Barlow, who is justly distinguished by his genius, high attainments, and long life devoted to the interests of science; but still it may not be out of place here to make one or two observations which occurred to me while reading the memoirs. I quite agree with Mr. Barlow, that there must be other forces

in operation when a beam is broken transversely than those simply and usually designated tensile and compressive. If a beam is broken transversely, and the existence and position of the neutral surface are admitted, then it is not difficult to conceive the existence of a third force between two adjacent laminæ unequally extended or compressed.

This is really what happens, and the existence of which was well known to Mr. Hodgkinson, who thought it to be so small in practical cases that its accumulated action would not produce much effect on the breaking strain of the beam. Be this, however, as it may, there is some little difficulty in subscribing to all that Mr. Barlow advances on this important and interesting subject. In the first place, there might be an exception taken to Mr. Barlow's method of fixing the position of the neutral line. Does he not fix it by an appeal to his senses rather than by the result of the mathematical analysis of the data he has obtained from experiment? The position which he fixes upon, viz. the centre of the beam, necessarily involves the equality of tensile and compressive forces—a conclusion which is not justified by Mr. Hodgkinson's experience. In the second place, Mr. Barlow makes it appear that the error in the breaking strain of a beam is nearly one-half, by neglecting the force of adhesion between the adjacent laminæ. We hardly think this conclusion is based upon sound premises, although it necessarily follows from the results of a formula which has been obtained by considering only the two forces, viz. tensile and compressive. But it is hardly fair on the part of Mr. Barlow to institute a comparison between the *resistance to flexure* and the results of a formula ($W = \frac{2}{3}adf \div l$) in which that *resistance to flexure* is neglected, without applying the well-known corrections to that formula. When a beam is strained to a considerable extent, the deflection becomes sensible, and of course the reaction at the supports, being perpendicular to the surface

of the beam, makes an angle with the vertical. This circumstance affects the above formula in two ways: 1st, it alters the amount of the moment about a line in the neutral surface; and 2nd, its tendency is to change the position of the neutral line. Therefore, unless these corrections are approximated to and applied to the formula, it is not safe to infer, as Mr. Barlow has done, that, by neglecting the *resistance to flexure*, the ordinary formula only gives nearly half the breaking weight.

Another source of error is in the law "ut tensio sic vis," as it is well known, from Mr. Hodgkinson's experiments, that the forces of extension and compression are neither equal nor vary with the extension and compression when the strains are large. I quite agree, as did Mr. Hodgkinson, with Mr. Barlow as to the existence of a *resistance to flexure* in the transverse strain of beams besides the ordinary forces of tension and compression; but the mode of estimating this *resistance to flexure* in Mr. Barlow's second memoir amounts to the assumption that the force of extension varies by a law expressed by $ax + b$, where a and b are constants, and x the distance of the particle from the neutral axis. I may add, in conclusion, that Mr. Hodgkinson has computed the tensile and compressive forces, subject to a law much more general than the one here alluded to, with great clearness and adaptation to include practical cases.

Mr. Barlow's two memoirs, however, are the first on this subject to insist on the existence of a distinct force to resist flexure; and although I do not see the force of his comparison of the *resistance to flexure* with the results of the ordinary formula, or the theoretical method by which he estimates its amount, still I can with confidence recommend these memoirs to the engineering student as being worthy of his attentive perusal.

In concluding this memoir of one of the most distin-

guished members of the Society, I cannot help feeling that the description herein given of his character and labours falls short of the real position which they occupy in the public mind; and although I have had much pleasure in reading and collating the discoveries of Mr. Hodgkinson, I regret that the preparation of this memoir has not been placed in abler hands. One thing, however, consoles me, and supplies me with an ample reward, which no criticisms on my effort can possibly cancel; and that is, I have been engaged, to the best of my ability, in the endeavour to perpetuate the memory of a great and good man, whose singular praise it is to have spent his life and his great powers for the good of mankind, with a single aim to truth and science, without desiring or gaining pecuniary reward.

XIV.—*On Non-modular Groups.* By the Rev. Thomas P. Kirkman, M.A., F.R.S., and Honorary Member of the Literary and Philosophical Societies of Manchester and Liverpool.

Read April 29th, 1862.

If we form with fifteen elements the five triplets

$$a\,a_1a_2,\quad b\,b_1b_2,\quad c\,c_1c_2,\quad d\,d_1d_2,\quad e\,e_1e_2,\quad .\quad .\quad \text{(E)}$$

the ten triplets

$$abc,\quad a\,d_2e_2,\quad a_1b_1e,\quad a_1c_1d_1,\quad a_2b_2d,\quad a_2c_2e_1,$$
$$b_1c_2d_2,\quad b_2c_1e_2,\quad b\,d_1e_1,\quad cde,\quad .\quad .\quad .\quad \text{(F)}$$

in which no two have the same three letters, nor any of them a duad before employed, and next the six quintuplets

$$\left.\begin{array}{l} a\,b_1c_1d\,e_1 \\ a\,b_2c_2d_1e \\ a_1b_2c\,d_2e_1 \\ a_1b\,c_2d\,e_2 \\ a_2b_1c\,d_1e_2 \\ a_2b\,c_1d_2e_1 \end{array}\right\} \quad . \; . \; . \; . \; . \; . \; . \quad (G)$$

in which we disregard for the present the order of the elements, we exhaust once, and once only, the duads of the fifteen elements. If next we define that the triplets (E) are triplets of mutually permutables, giving $1 = aa_1a_2 = bb_1b_2$, &c., that the triplets (F) are didymous factors of ten substitutions of the third order $\alpha\,\beta\,\gamma\,\delta\,\epsilon\,\zeta\,\eta\,\theta\,\lambda\,\mu$, and that the quintuplets (G) are also didymous factors of six substitutions $\nu\,\rho\,\sigma\,\tau\,\phi\,\omega$ of the fifth order, we can easily prove that every triplet not exhibited, and possible with with the fifteen elements, reduces to a duad. We assume that the fifteen elements are all of one form; then it is sufficient to prove this of the triplets beginning with a, as all the fifteen are alike involved.

It is evident that any triplet containing the same letter in its first or final duad reduces by (E). The triplets aba, aba_1, aba_2 are

$$aba = aac = c,\ aba_1 = caa_1 = ca_2,$$
$$aba_2 = caa_2 = ca_1, \text{ all by (F) and (E).}$$

The triplet $abc_1 = bcc_1 = bc_2$, by (F) and (E),
The triplet $ab_1c_2 = b_1c_1c_2 = b_1c$, by (G) and (E);

and thus every triplet can be shown to be reducible.

We have, then, by adding to the fifteen elements the twenty substitutions $\alpha\alpha^2$, $\beta\beta^2$, &c., and the twenty-four substitutions $\nu\nu^2\nu^3\nu^4$, $\rho\rho^2\rho^3\rho^4$, &c., a group with unity, of

$$20_3 + 24_5 + 15_2 + 1 = 60.$$

Such a group, non-modular, is obtained by selecting from

the entire group of 120, made with 12345, the sixty positive substitutions. *Vide* the preceding volume, p. 283.

Take now the ten elements 1234567890. First we are to take aa_1a_2 mutually permutable, and such that bb_1 being permutable, ab_1 shall be of the fifth, and ab of the third order. Mutually permutables are either

$$\left.\begin{array}{l} 1234567890 \\ 1325468709 = a \\ 1543260987 = (a_1) \\ 1452369078 = (a_2), \end{array}\right\} (\mathrm{H}), \text{ or } \left.\begin{array}{l} 1234567890 \\ 1325468709 = a \\ 6324510987 = a_1 \\ 6235419078 = a_2\,; \end{array}\right\} (\mathrm{H}')$$

and any of the substitutions $aa_1a_2(a_1)(a_2)$ may form part of a didymous system of 5, as may be seen if we form the didymous radicals of 2345178906. Any of them may also form part of a didymous system of 3. This requires to be shown thus. The following,

$$\begin{array}{ll} 1234567890 & 1324657980 \\ 2315648970 & 3216549870 \\ 3126459780 & 2135468790, \end{array}$$

is a group of 6 of my theorem G (art. 26). The didymous radicals have here all four elements undisturbed. If now we exchange the two last elementary groups thus, we have, (art. 46) of my memoir in the last volume,

$$\begin{array}{llll} 132 & 798 & 465 & 0 \\ 321 & 987 & 654 & 0 \\ 213 & 879 & 546 & 0, \end{array}$$

a system of didymous radicals of the same substitution 2315648970, and all of the form of aa_1a_2, which have only two elements undisturbed.

The above two groups H H′ are curious, as proving that two groups may be alike in the number and orders both of their substitutions and of their circular factors, and yet not be equivalent groups; for every equivalent of the first will have two vertical rows of elements undisturbed.

If we begin with the first system, $a(a_1)(a_2)$, we shall merely reproduce the non-modular group of 60 above mentioned made with 12345, and the same group made with 67890, which will teach us nothing.

We therefore begin with aa_1a_2 above written, which have not all the same undisturbed elements. The first thing required is b_1 such that ab_1 and a_2b_1 shall be of the fifth, and a_1b_1 shall be of the third order. We write

$$a=1325468709,\quad a_1=6324510987,$$
$$a_2=6235419078,\quad b_1=pqrstuvwxy.$$

As a_1b_1 is to have circular factors of three elements, we shall have either $t=5$, or $s=4$. Put $t=5$, and try the vertical circles 412, 413, &c., till our problem is either solved or proved impossible. The circle 412 gives $b_1=pq4152\ldots$, which gives, when written under a, a circle of 5 only on condition that $p=6$, which is inadmissible, for by (G) a_2 and b_1 can have no element in the same place. The same objection lies against the circle $41m$, whatever m may be. After a few trials we find that 407 is a proper circle of a_1b_1; and we readily complete the systems

$$a_1=6324510987$$
$$b_1=9860537214$$
$$e=2197584630,$$

$$a=1325468709\qquad a_2=6235419078$$
$$b_1=9860537214\qquad b_1=9860537214$$
$$e_1=4731082695\qquad e_2=7492061835$$
$$c=5289176340\qquad d_1=1578293460$$
$$d=0674923851,\qquad c_1=3014876592;$$

where ee_1e_2 are mutually permutables, as are cc_1 and dd_1. We have next

$$d_2=dd_1=0938657421,$$
$$c_2=cc_1=8059367142.$$

To find b, b_2, we write c_1 under a, which will give either a triplet or a quintuplet containing a. It turns out to be a

quintuplet, by completing the vertical circle:

$$
\begin{aligned}
a &= 1325468709\\
c_1 &= 3014876592\\
d_2 &= 0938657421\\
b &= 9206745813\\
e &= 2197584630.
\end{aligned}
$$

Next

$$b_2 = bb_1 = 1843705296,$$

which completes our fifteen substitutions of the second order, which are all of the same form.

The substitutions of the third order are

$$
\begin{array}{ll}
\alpha = a_1 b_1 = 8917520364, & \alpha^2 = 3680594127 = a_1 e,\\
\beta = a_1 c_1 = 2764901583, & \beta^2 = 7104832956 = a_1 d,\\
\gamma = a\ c_2 = 7940268153, & \gamma^2 = 8503961724 = a\ e_2,\\
\delta = a\ b_2 = 1752894306, & \delta^2 = 1487302569 = a\ d_1,\\
\epsilon = a_2 b = 7281954063, & \epsilon^2 = 4207691358 = a_2 c,\\
\zeta = a_2 e_1 = 5936802174, & \zeta^2 = 8730149526 = a_2 d_2,\\
\eta = b_1 d_2 = 4162357089, & \eta^2 = 2451637908 = b_1 c_2,\\
\theta = b_2 c_1 = 4613250798, & \theta^2 = 3541628097 = b_2 e_1,\\
\lambda = c\ d_1 = 5163248970, & \lambda^2 = 2546139780 = c\ e,\\
\mu = b\ e_2 = 5612349807, & \mu^2 = 3456120879 = b\ d.
\end{array}
$$

Three more quintuplets are

$$
\begin{aligned}
a_1 &= 6324510987\\
b_2 &= 1843705296\\
d_2 &= 0938657421\\
c &= 5289176340\\
e_2 &= 7492061835,
\end{aligned}
$$

$$
\begin{array}{ll}
a_1 = 6324510987 & a_2 = 6235419078\\
b = 9206745813 & b_2 = 1843705296\\
c_2 = 8059367142 & d = 0674923851\\
d_1 = 1578293460 & e = 2197584630\\
e_1 = 4731082695, & c_2 = 8059367142.
\end{array}
$$

The powers of ab_1, a_2b_1, ac_1, a_1b_2, a_1b, a_2b_2 are the twenty-

four substitutions of the fifth order, which complete the non-modular group of 60.

The triplets (F) and the quintuplets (G) are correctly written thus, with changes of subindices only,

$$ab_2d_1,\quad ac_2e_2,\quad a_1b_1e,\quad a_1c_1d,\quad a_2bc,\quad a_2e_1d_2,\quad b_1c_2d_2,\quad b_2c_1e_1,\quad bde_2,\quad cd_1e,$$

$$ab_1e_1cd,\quad ac_1d_2be,\quad a_1b_2d_2ce_2,\quad a_1b\,c_2d_1e_1,\quad a_2b_1e_2d_1c_1,\quad a_2b_2dec_2,$$

in the exact order of their succession as didymous radicals.

It is remarkable that this group (J) of 60 differs not in the number and form of its substitutions from the group of 60 made with 12345 written parallel in a column with the same group made with 67890; but it is not, for all that, equivalent to the latter. The latter is intransitive; for 1 could appear only in vertical circles of five made with 12345, but in this group last constructed 1 is found in a vertical circle of five with every other element, and the group is transitive as well as non-modular.

This group of 60 is given analytically by M. Betti in M. Hermite's 'Théorie des Equations Modulaires,' Paris. I know not whether its first discovery is due to Galois, Betti, or Kronecker.

Take fifteen elements, $a_1a_2a_3$, $b_1b_2b_3$, $c_1c_2c_3$, $d_1d_2d_3$, $e_1e_2e_3$, forming five triplets, and ten capitals, ABCDEFGHIJ. It is easy to form the fifteen triplets following:—

$$\left.\begin{array}{l} \mathrm{AB}a_1 \\ \mathrm{AF}b_1\ \ \mathrm{BC}d_1\ \ \mathrm{FE}e_2\ \ \mathrm{ID}e_3\ \ \mathrm{CD}b_2\ \ \mathrm{GJ}b_3 \\ \mathrm{AI}c_1\ \ \mathrm{BG}e_1\ \ \mathrm{FH}d_2\ \ \mathrm{IJ}d_3\ \ \mathrm{CE}c_2\ \ \mathrm{GH}c_3\ \ \mathrm{EJ}a_2\ \ \mathrm{HD}a_3\,; \end{array}\right\} \quad . \quad (\mathrm{R})$$

where the fifteen small elements are once employed, where the capitals are any fifteen duads showing each of the ten letters AB ... J three times, and where the small letters are combined with them in any way so as to answer the condition that the five triplets in which X and Y occur shall

exhibit five different small letters, disregarding the sub-indices. Thus the five triplets

$$AFb_1 \quad FEe_2 \quad FHd_2 \quad CEc_2 \quad EJa_2,$$

in which F and E occur, exhibit five different small letters.

The triplets (R) give us the fifteen quadruplets

$$\left.\begin{array}{lllll} Aa_2Ba_3 & Bd_2Cd_3 & Fd_1Hd_3 & Cb_1Db_3 & Gc_1Hc_2 \\ Ab_2Fb_3 & Be_2Ge_3 & Ie_1De_2 & Cc_1Ec_3 & Ea_1Ja_3 \\ Ac_2Ic_3 & Fe_1Ee_3 & Id_1Jd_2 & Gb_1Jb_2 & Ha_1Da_2, \end{array}\right\} . \quad (Q)$$

as also the ten sextuplets, formed from the three triplets containing A or B, &c.

$$\left.\begin{array}{llll} Bc_1Fa_1Ib_1 & Ae_3Jc_1Dd_3 & Fc_2Je_2Ca_2 & Ia_2Gd_3Eb_3 \\ Ae_1Ca_1Gd_1 & Bc_2Dd_1Eb_2 & Ia_3Ce_3Hb_2 & \\ Ad_2Eb_1He_2 & Bc_3Je_1Hb_3 & Fa_3Gd_2Dc_3. & \end{array}\right\} . \quad (S)$$

In (Q) and (S) we have exhausted all the duads of the ten capitals, and all duads made with a capital and any small element. The duads of small letters not yet employed are found in the six quintuplets,

$$\left.\begin{array}{lll} a_1b_3c_2d_2e_3 & a_2b_2c_1d_2e_1 & a_3b_3c_1d_1e_2 \\ a_1b_2c_3d_3e_2 & a_2b_1c_3d_1e_3 & a_3b_1c_2d_3e_1; \end{array}\right\} . \quad (T)$$

and this is the only way in which they can be combined so as to repeat no duad.

We have exhausted all our duads of our 15 + 10 elements in (Q), (S), (T). Let us define that the quadruplets (Q) are didymous radicals of fifteen groups of the fourth order, the 6-plets (S) those of ten groups of the sixth, and the 5-plets (T) those of six groups of the fifth order. There will be (art. 79) a substitution of the second order, $\theta^2{}_4$ or $\phi^3{}_6$, permutable with any four in (Q), or any six in (S); and as we see that a_2a_3, a_4a_1, a_1a_2 are all permutables in Q, we have in our first triplets $a_1a_2a_3 = 1 = b_1b_1b_2 =$ &c., and each of these fifteen small letters is θ^2 permutable with a set of (Q). And as $a_1AB = 1$ in the first 4-plet, we have A and B for the sub-

stitutions $\phi^3{}_6$, $\chi^3{}_6$ permutables with the first two 6-plets. Hence all the triplets (R) are permutable sets,

$$AB a_1 = 1 = AF b_1, \text{ \&c.}$$

The letters AB, &c. are those which we dropped in forming the sextuplets.

The triplets possible with the twenty-five elements are next to be examined. We have to consider the forms ABC, *abc*, A*bc*, AB*c*, *b*A*c*, *bc*A, A*c*B, *c*AB. Let the triplet be ABC; $AB = a_1$ or $BC = d_1$, giving two ways of reduction; and thus every triplet of three capitals is a duad. The triplet $a_1 b_2 c_1$ has its first duad in (T), and we have the reduction $a_1 b_2 . c_1 = b_2 c_3 . c_1 = b_2 c_2$. The triplet $a_1 b_1 c_2$ has its second duad in (T), and we have $a_1 . b_1 c_2 = a_1 . a_3 b_1 = a_2 b_1$. The triplet $a_1 b_1 c_1$ in (S) is FIB, reduced before.

$$ABc_1 = a_1 c_1 \text{ by (Q)}, \quad Bc_1 A = Fa_1 A = FB_1 \text{ by (S)(Q)},$$
$$c_1 AB = c_1 a_1.$$
$$a_1 b_1 C = FIC, \; a_1 b_2 C = a_1 D, \; a_1 b_3 C = b_3 c_2 C = b_3 E.$$

Thus every triplet may be proved reducible. It will be useful to lay down the conditions of reduction of any triplet of radicals xyz, having any elements capital or small. Let $\{xy\}$, $\{xz\}$, or $\{yz\}$ be the multiplet containing the duad xy, xz, or yz. Then xyz is reducible,

(1) if $\{xy\}$ contains z' permutable with z, for we have

$$xyz = tz'z = tz'';$$

(2) if $\{yz\}$ contains x' permutable with x, for we have

$$xyz = xx't = x''t;$$

(3) if $\{xz\}$ contains u' permutable with $zyz = u$, for

$$xyz = xzzyz = xzu = tu'u = tu''.$$

Here $uz = zy$, or u is in $\{zy\}$ equidistant from z with y.

It is easy to see that, if none of these conditions is fulfilled, the triplet xyz is irreducible.

If now we add to the 25 square roots of unity $a_1 a_2 a_3$, &c.,

ABC, &c., the 30 substitutions of the fourth order, $Aa_2=\theta$, $Aa_3=\theta^3$, &c., given by (Q), the 20 of the sixth order $Bc_1=\phi$, $Bb_1=\phi^5$, &c., and the 20 of the third order $BF=\phi^2$, $BI=\phi^4$, &c., given by (S), and the 24 of the fifth order $a_1b_3=\rho$, $a_1c_2=\rho^2$, &c., given by (T), we shall complete a group of

$$20.1_6+24.1_5+30.1_4+20.1_3+25.1_2+1=120.$$

To construct such a group let us take six symbols, 123456. We see by the first quadruplet and sextuplet that we have to take Ba_3c_1 such that a_3B shall be of the fourth, c_1B of the sixth, and c_1a_3 (in the third quintuplet) of the fifth order, and that a_1 permutable with d_3 shall be the fourth in the 6-plet Bc_1.

All the didymous radicals of ρ of the fifth order made with six elements will have a common element undisturbed, and each a different second element undisturbed. We take at random

$c_1=165432$, giving c_1a_3 of the fifth order.
$a_3=154326$.

It follows from the theorem (art. 77 *corrected, vide* art. 76), *Whenever the order* K *of the group* $1+\phi+\phi^2+\ldots$, *made on the partition*

$$N=N.1,$$

or

$$N=M.1+1.2,$$

or

$$N=(N-a).1+1.a,$$

is even, the didymous factors of ϕ^{2m} *are two of the same form, but those of* ϕ^{2m+1} *are two of different forms*; that B is not of the form of c_1: it will have no letter undisturbed. We cannot make a_3B of the fourth order, if 61 is one of the three transpositions of B; for by the above theorem applied to a circle of $N=4$, we must have 1 and 6 in the circle of 4. Try for a_3B the vertical circles 1265 and 1364. We get

a_3 154326	$c_1=165432$
B 214365	$B=214365$
a_2 624351	$b_1=523614$
A 564312	$I=456123$
	$a_1=341256$
	$F=632541$

a_3 154326	$c_1=165432$	$c_1=165432$
B 351624	$B=351624$	$a_3=154326$
a_2 653421	$b_1=213546$	$a_1=143265$
A 456123	$I=432165$	$e_2=132654$
	$a_1=624351$	$b_3=126543$
	$F=546213$	

The first found a_1 is not, but the second a_1 is, permutable with a_3; wherefore $B=351624$ is correct.

We have a_1b_3, a_1e_2, a_2c_1, a_2d_1, a_3b_1; that is, we have the remaining five quintuplets

$a_1=624351$	$a_1=624351$	$a_2=653421$
$b_3=126543$	$e_2=132654$	$d_1=143265$
$d_2=321465$	$c_3=463152$	$e_3=523614$
$e_3=523614$	$b_2=216453$	$c_3=463152$
$c_2=425136$	$d_3=341256$	$b_1=213546$

$a_3=154326$	$a_2=653421$
$b_1=213546$	$c_1=165432$
$c_2=425136$	$b_2=216453$
$d_3=341256$	$d_2=321465$
$e_1=532416$	$e_1=532416$.

Next we have $C=Bd_1$, $G=Be_1$, $E=Fe_2$, $H=Fd_2$, $D=Ie_3$, $J=Id_3$, and the whole of the twenty-five substitutions of the second order; and with them the entire group of 120 are determined. This is one of the non-modular groups of art. 65 of my memoir above quoted.

I do not see that this mode of investigating this group of 120 adds much to our knowledge of these groups; but

it is perhaps worth while to have spent so much time on the method, for the sake of showing that we can dispense in the construction of these groups with congruences, and with the imaginary subindices so ably handled by MM. Betti and Mathieu. Besides this, it may not be useless to show the connexion between the theory of groups and that of combinations ; and the theorem that we have proved, *that the duads made with* 25 *elements can be exhausted in ten* 6-*plets, six* 5-*plets, and fifteen* 4-*plets,* is of itself deserving of record and of an example.

The theory of combinations appears to me, with my present light, to be likely to owe more than it can contribute to that of groups. The theorem about the duads of 25 is obtained by the study of the group of 120 made with five, or of M. Mathieu's group of 120 made with six elements. It is a simple case of this more general proposition (proved by inspection of the groups of his general theorem), that groups of $(\mathrm{N}+1)\mathrm{N}\,.\,(\mathrm{N}-1)$ can always be formed with $\mathrm{N}+1$ elements when N is prime :

Theorem: *When* N *is any prime number,* N^2 *elements can be thrown into* $\frac{1}{2}\mathrm{N}\,.\,(\mathrm{N}+1)(\mathrm{N}-1)$ $(\mathrm{N}-1$-*plets*$)$, $\mathrm{N}+1$ N-*plets, and* $\frac{1}{2}\mathrm{N}\,.\,(\mathrm{N}-1)$ $(\mathrm{N}+1)$-*plets, so as to exhaust once and once only the duads possible with the* N^2 *elements.*

I shall content myself with giving the twenty-eight 6-plets, the eight 7-plets, and the twenty-one 8-plets, which can thus be made with the $28+21=49$ elements,

$a\,b\,c\,d\,e\,f\,g\,h\,i\,j\,k\,l\,m\,n\,p\,q\,r\,s\,t\,u\,v\,w\,x\,y\,z\,\alpha\,\beta\,\gamma,$
ABCDEFGHIJKLMNPQRSTUV.

$b\mathrm{C}c\mathrm{A}d\mathrm{B}$	$g\mathrm{Q}x\mathrm{B}y\mathrm{K}$	$x\mathrm{V}d\mathrm{N}u\mathrm{L}$	$t\mathrm{P}h\mathrm{H}l\mathrm{Q}$
$a\mathrm{D}l\mathrm{A}k\mathrm{E}$	$\gamma\mathrm{C}\beta\mathrm{S}i\mathrm{L}$	$e\mathrm{I}m\mathrm{J}w\mathrm{P}$	$i\mathrm{F}k\mathrm{Q}u\mathrm{T}$
$a\mathrm{G}m\mathrm{B}n\mathrm{F}$	$w\mathrm{T}b\mathrm{U}\alpha\mathrm{D}$	$\gamma\mathrm{T}e\mathrm{G}p\mathrm{K}$	$l\mathrm{G}j\mathrm{V}s\mathrm{S}$
$a\mathrm{H}q\mathrm{C}p\mathrm{I}$	$\gamma\mathrm{P}b\mathrm{V}y\mathrm{E}$	$f\mathrm{H}n\mathrm{L}y\mathrm{U}$	$j\mathrm{D}m\mathrm{L}x\mathrm{R}$
$f\mathrm{J}s\mathrm{A}r\mathrm{K}$	$r\mathrm{F}\beta\mathrm{P}c\mathrm{R}$	$q\mathrm{F}f\mathrm{V}\alpha\mathrm{M}$	$g\mathrm{I}k\mathrm{S}r\mathrm{U}$
$e\mathrm{L}u\mathrm{A}t\mathrm{M}$	$c\mathrm{N}t\mathrm{G}z\mathrm{U}$	$g\mathrm{E}p\mathrm{M}z\mathrm{R}$	$v\mathrm{J}n\mathrm{E}i\mathrm{N}$
$h\mathrm{M}c\mathrm{B}v\mathrm{S}$	$v\mathrm{T}d\mathrm{R}s\mathrm{H}$	$h\mathrm{D}q\mathrm{K}s\mathrm{N}$	$j\mathrm{J}\alpha\mathrm{C}z\mathrm{Q}.$

aQeNbRfS
aLgVcThJ
aKiPdUjM

tEjTfIβB	αAyGiRhI	vDfCgGuP
tVkCnRwK	αPpLkNsB	vAβUpQmV
tDpFyJdS	αEcHuKmS	vFlIzKbL
xCsEhUeF	γBuRlJqU	rBzHeViD
xAzPqSnT	γFgAjHeN	rTlCmNyM
xMkJsGbH	γDcIsMnQ	rEqGwLdQ

aγtxvrα	*γdzhkmf*	*xwcfpli*	*rbujnpk*
auβywzs	*tbsimqg*	*vcyeqkj*	*αβledgn.*

Every duad of the forty-nine elements is once and once only employed. If these be read as systems of didymous factors, it is easy to prove by inspection that every triplet made with the forty-nine elements is reducible; for there is no multiplet which does not contain a letter permutable with a letter of every other multiplet, and every octuplet has a letter θ permutable with every letter θ' not in the octuplet, such that $\theta'\theta=\theta''=\theta\theta'$. The key to the above multiplets is the following system of 14+21 triplets, each being a set of three mutually permutables, in which system every capital is found four times, and every small letter thrice.

AQR	SFH	NTP	ILE	GDM	VBJ	UCK
ASN	QIG	RUV	FEK	HDJ	TBL	PCM,

abA	acB	adC	γlV	γjS	γsG	vgM	
efA	ghB	ijC	uqV	gwS	cnG	fuM	
vpR		vzE	tfL	tnU	tyH	xhK	
βmR		lbE	jβL	kwU	pdH	seK	
xqN	xβD	αiQ	αkT	αuF	reJ	rmP	rwI
znN	kbD	yhQ	psT	cmF	ziJ	lyP	qdI.

We have now to construct this group of 8 . 7 . 6. We observe that the first octuplet has its elements all permu-

table with $A=ab=QR$, &c., and that the first sextuplet has all its elements permutable with $a=bA=Cd$, &c., as read in the triplets AQR, Aab, adC, &c. We see also, looking at the 8-plet and 6-plet, that df in the third and ce in the sixth septuplet are substitutions of the seventh order.

We may take at pleasure any system of didymous factors of a substitution of the eighth order. Take then

$$1\,8\,3\,6\,7\,4\,5\,2 = a$$

$$\begin{aligned}
5\,3\,2\,7\,1\,8\,4\,6 &= Q\\
4\,2\,6\,1\,5\,3\,8\,7 &= e\\
8\,6\,7\,5\,4\,2\,3\,1 &= N\\
3\,7\,1\,4\,8\,6\,2\,5 &= b\\
2\,1\,5\,8\,3\,7\,6\,4 &= R\\
6\,5\,4\,3\,2\,1\,7\,8 &= f\\
7\,4\,8\,2\,6\,5\,1\,3 &= S.
\end{aligned}$$

We have $A=ab=35162487$ and we have to form the system

$$\begin{aligned}
1\,8\,3\,6\,7\,4\,5\,2 &= a\\
3\,5\,1\,6\,2\,4\,8\,7 &= A\\
a\,b\,f\,g\,h\,i\,j\,k &= d\\
l\,m\,n\,o\,p\,q\,r\,s &= B\\
3\,7\,1\,4\,8\,6\,2\,5 &= b\\
\ldots\ldots\ldots &= C\\
\ldots\ldots\ldots &= c,
\end{aligned}$$

where AdBbCc are all permutable with a.

It is plain that the only possible vertical circles under 3 and 1 in A are 333.. and 111... We have then

$$\begin{aligned}
3\,5\,1\,6\,2\,4\,8\,7 &= A\\
3\,b\,1\,g\,h\,i\,j\,k &= d\\
3\,m\,1\,o\,p\,q\,r\,s &= B\\
3\,7\,1\,4\,8\,6\,2\,5 &= b\\
\ldots\ldots\ldots &= C\\
\ldots\ldots\ldots &= c.
\end{aligned}$$

Let us suppose $b=8$ in d; then $k=2$: what is m in the circle $58m7$..? It follows 8; wherefore $m=j$, and j is the letter preceding 7 in the circle $58j7$... Now j, in $d=381ghij2$ and in the circle $58j7$..., can be none of 123578. If $j=6$ precedes 7, $i=7$, and 4 precedes 7; if $j=5$ precedes 7, $h=7$, and 2 precedes 7; if $j=4$ precedes 7, $g=7$, and 6 precedes 7: all absurd.

Therefore b is not 8 in d, and in the circle $5bm7$... Try $b=6$: then $i=2$, and m precedes 7 in the circle $56m7$..., wherefore m follows 6, or $m=g$. We have now $d=361mh2jk$. If $m=4=g$ in B, we have the absurd vertical circle 6424..; and if $m=8=g$, $k=4$, and $h=5$ and $j=7$ of necessity; for d, b, and c must have each two elements undisturbed. This gives

$$
\begin{array}{l}
3\,5\,1\,6\,2\,4\,8\,7 = \mathrm{A} \\
3\,6\,1\,8\,5\,2\,7\,4 = d \\
3\,8\,1\,7\,6\,5\,4\,2 = \mathrm{B} \\
3\,7\,1\,4\,8\,6\,2\,5 = b \\
3\,4\,1\,2\,7\,8\,5\,6 = \mathrm{C} \\
3\,2\,1\,5\,4\,7\,6\,8 = c.
\end{array}
$$

We have to examine df and ce: these are

$$
\begin{array}{ll}
d = 36185274 & c = 32154768 \\
f = 65432178 & e = 42615387 \\
k = 52861473 & k = 52861473 \\
z = 21354876 & v = 12786534 \\
\gamma = 14628375 & y = 62378145 \\
m = 48513672 & q = 82437651 \\
h = 83246571, & j = 72543816,
\end{array}
$$

systems of seven as they ought to be. We have now all the 49 elements in our power. We thus complete the list of capitals and small letters, of which we yet require

$gilnprstuwx\alpha\beta$, DEFGHIJKLMPTUV.

$g = h\mathrm{B} = 21875643$	$\mathrm{D} = kb = 87563421$
$i = j\mathrm{C} = 54721638$	$\mathrm{E} = vz = 21768435$
$l = b\mathrm{E} = 73265418$	$\mathrm{F} = cm = 58431762$
$n = z\mathrm{N} = 68745132$	$\mathrm{G} = cn = 78654312$
$p = v\mathrm{R} = 21647358$	$\mathrm{P} = ly = 43218765$
$r = m\mathrm{P} = 15842763$	$\mathrm{T} = \mathrm{NP} = 57681324$
$s = \gamma\mathrm{G} = 75382614$	$\mathrm{L} = \mathrm{BT} = 64523187$
$t = f\mathrm{L} = 13254687$	$\mathrm{H} = \mathrm{SF} = 63287154$
$u = f\mathrm{M} = 56341287$	$\mathrm{I} = \mathrm{QG} = 46817253$
$w = g\mathrm{S} = 47316528$	$\mathrm{J} = \mathrm{HD} = 45712836$
$x = q\mathrm{N} = 16573248$	$\mathrm{K} = \mathrm{FE} = 85672341$
$\alpha = i\mathrm{Q} = 17435826$	$\mathrm{M} = \mathrm{CP} = 21436587$
$\beta = j\mathrm{L} = 84325761,$	$\mathrm{U} = \mathrm{CK} = 67854123$
	$\mathrm{V} = \mathrm{BJ} = 76438215.$

Nothing is easier now than to construct the remainder of the group of 8 . 7 . 6. Thus

*a*D and its powers are

12345678
25743681
53847612
37148625
78241653
81542637.

xc and its powers are

12345678
37461258
45623718
61274538
23756148
74512368
56137428.

The entire non-modular group of 8 . 7 . 6 is thus written :—

Substitutions of the Second Order.

A 35162487			
B 38176542	G 78654312	L 64523187	R 21583764
C 34127856	H 63287154	M 21436587	S 74826513
D 87563421	I 46817253	N 86754231	T 57681324
E 21768435	J 45712836	P 43218765	U 67854123
F 58431762	K 85672341	Q 53271846	V 76438215

a 18367452	h 83246971	p 21647358	w 47316528
b 37148625	i 54721638	q 82437651	x 16573248
c 32154768	j 72543816	r 15842763	y 62378145
d 36185274	k 52861473	s 75382614	z 21354876
e 42615387	l 73265418	t 13254687	α 17435826
f 65432178	m 48513672	u 56341287	β 84325761
g 21875643	n 68745132	v 12786534	γ 14628375

Substitutions of the Eighth Order.

$a\mathrm{Q}=\delta=73851264$,	$\delta^3=24576831$,	$\delta^5=81723546$,	$\delta^7=56284713$,
$a\mathrm{L}=\epsilon=46783125$,	$\epsilon^3=54632817$,	$\epsilon^5=75421386$,	$\epsilon^7=67518234$,
$a\mathrm{K}=\eta=27458361$,	$\eta^3=63812547$,	$\eta^{5'}=45276183$,	$\eta^7=81634725$,
$t\mathrm{E}=\theta=31867524$,	$\theta^3=48671235$,	$\theta^5=56718342$,	$\theta^7=27186453$,
$t\mathrm{V}=\iota=86527314$,	$\iota^3=25138746$,	$\iota^5=31472865$,	$\iota^7=74683251$,
$t\mathrm{D}=\kappa=78462531$,	$\kappa^3=47521863$,	$\kappa^5=54813726$,	$\kappa^7=85736412$,
$x\mathrm{C}=\lambda=57164832$,	$\lambda^3=61428753$,	$\lambda^5=24837165$,	$\lambda^7=38751426$,
$x\mathrm{A}=\mu=53126784$,	$\mu^3=75618423$,	$\mu^5=47862315$,	$\mu^7=34281567$,
$x\mathrm{M}=\nu=61752384$,	$\nu^3=73416852$,	$\nu^5=48237516$,	$\nu^7=25684137$,
$\alpha\mathrm{A}=\xi=45187362$,	$\xi^3=26853417$,	$\xi^5=71564283$,	$\xi^7=38612754$,
$\alpha\mathrm{P}=\pi=34716285$,	$\pi^3=83574162$,	$\pi^5=68253741$,	$\pi^7=46128537$,
$\alpha\mathrm{E}=\rho=71286345$,	$\rho^3=84762153$,	$\rho^5=65827431$,	$\rho^7=23678514$,
$\gamma\mathrm{B}=\sigma=65173824$,	$\sigma^{3'}=41856732$,	$\sigma^5=28714563$,	$\sigma^7=37582146$,
$\gamma\mathrm{F}=\tau=85261734$,	$\tau^3=68134257$,	$\tau^5=36457182$,	$\tau^7=53782461$,
$\gamma\mathrm{D}=\upsilon=57836241$,	$\upsilon^3=28714563$,	$\upsilon^5=41263857$,	$\upsilon^7=86471523$,
$v\mathrm{D}=\phi=43657821$,	$\phi^3=78123465$,	$\phi^5=34568712$,	$\phi^7=87214356$,
$v\mathrm{A}=\chi=76152843$,	$\chi^3=53468127$,	$\chi^5=67231485$,	$\chi^6=35874216$
$v\mathrm{F}=\psi=64871352$,	$\psi^3=85413267$,	$\psi^5=46532781$,	$\psi^7=58627143$,
$r\mathrm{B}=\omega=83167245$,	$\omega^3=78536124$,	$\omega^5=67483512$,	$\omega^7=36278451$,
$r\mathrm{T}=\alpha_1=26731854$,	$\alpha_1^3=84156327$,	$\alpha_1^5=37624581$,	$\alpha_1^7=51487236$,
$r\mathrm{E}=\beta_1=51673482$,	$\beta_1^3=63724815$,	$\beta_1^5=74258136$,	$\beta_1^7=28561347$.

Substitutions of the Fourth Order.

$\delta^2=68417325$,	$\delta^6=47638152$,	$\epsilon^2=81257463$,	$\epsilon^6=23864751$,
$\eta^2=76581432$,	$\eta^6=58763214$,	$\theta^2=83425716$,	$\theta^6=75234861$,
$\iota^2=47361582$,	$\iota^6=58216437$,	$\kappa^2=31658247$,	$\kappa^6=26174385$,
$\lambda^2=43586217$,	$\lambda^6=76213584$,	$\mu^2=61537842$,	$\mu^6=28473156$,
$\nu^2=36821745$,	$\nu^6=54178263$,	$\xi^2=87426135$,	$\xi^6=64738521$,
$\pi^2=71832456$,	$\pi^6=25467813$,	$\rho^2=47153286$,	$\rho^6=36514827$,
$\sigma^2=83621454$,	$\sigma^6=54267381$,	$\tau^2=41578326$,	$\tau^6=68572413$,
$\upsilon^2=64182735$,	$\upsilon^6=35748162$,	$\phi^2=56872134$,	$\phi^6=65781243$,
$\chi^2=48726351$,	$\chi^6=84617532$,	$\psi^2=37256814$,	$\psi^6=73184526$,
$\omega^2=51824367$,	$\omega^6=24631783$,	$\alpha_1^2=68572413$,	$\alpha_1^6=75863142$,
		$\beta_1^2=35486721$,	$\beta_1^6=87132564$.

Substitutions of the Sixth Order bC and $(b\mathrm{C})^5$, &c.

*b*C,	14372586,	15326847;	*a*D,	25743681,	81542637;
*a*G,	52476318,	72631548;	*a*H,	43825176,	64215873;
*f*J,	32765841,	82175436;	*e*L,	31526478,	24163578;
*h*M,	38425617,	74135682;	*g*Q,	58142376,	35641872;
γC,	62147583,	32846157;	*w*T,	62584371,	82653174;
γD,	26415738,	41735268;	*r*F,	23481675,	51238674;
*c*N,	87645213,	76845321;	*v*T,	65341728,	57342168;
*x*V,	42758613,	72814635;	*e*I,	13748256,	16247835;
γT,	87351642,	58374621;	*f*H,	14587623,	17823654;
*q*F,	71348562,	28346715;	*g*E,	12463785,	12538467;
*h*H,	52317864,	42381756;	*t*P,	52317864,	42381756;
*i*F,	18275364,	13685742;	*l*G,	18456273,	16834572;
*j*D,	61385427,	27365184;	*g*I,	76324158,	64357218;
*v*J,	86312475,	45368271;	*j*J,	43172658,	35217648.

Substitutions of the Third Order $(b\mathrm{C})^2$, $(b\mathrm{C})^4$, &c.

*b*C,	17384265,	16358724;	*a*D,	53847612,	78241653;
*a*G,	62713458,	42567138;	*a*H,	28635471,	81465372;
*f*J,	72485163,	62835714;	*e*L,	53614278,	46251378;
*h*M,	47285631,	83715624;	*g*Q,	26548173,	61843275;
γC,	52648731,	82741365;	*w*T,	32418576,	42136875;
γP,	67125348,	34675128;	*r*F,	34852671,	85124673;
*c*N,	31245786,	23145867;	*v*T,	71346258,	57342168;
*x*V,	52183647,	32571684;	*e*I,	17546382,	18643527;
γT,	24318657,	41327685;	*f*H,	17546382,	18643527;
*q*F,	67342851,	85347126;	*g*E,	12674853,	12857346;
*h*H,	72356481,	82364517;	*t*P,	72356481,	82364517;
*i*F,	14865237,	16725483;	*l*G,	13562874,	15283476;
*j*D,	46375812,	78315246;	*g*I,	51362748,	25371468;
*v*J,	54386172,	68321574;	*j*J,	71453628,	27534618.

All M. Mathieu's groups of $(N_i+1)N^i.(N^i-1)$, when N is any prime number, can be thus discussed and constructed without the aid of congruences. And the triplets of all the didymous factors are reducible to duads. This may perhaps ripen into a complete tactical theory of groups.

In order that the construction of the group of 8 . 7 . 6 should *evidently* follow from the notation of the above multiplets, it would be necessary (and it would not be difficult) to treat the matter from the beginning in a manner something like the following mode of discussing two groups of considerable interest, of 7 . 6 . 4 and 11 . 10 . 6.

From the seven triads which exhaust the duads of seven elements, namely

157, 261, 372, 413, 524, 635, 746,

we can form twenty-one triads thus, each containing a capital and two small figures,

157, 571, 315, 261, 612, 126, &c.

We can collect the triplets of these triads which contain the same small figures thus, the order of the small figures being indifferent:—

$$\left.\begin{array}{ccccccc} 157 & 157 & 517 & 517 & 571 & 571 & 751 \\ 327 & 237 & 327 & 237 & 431 & 341 & 431 \\ 467 & 647 & 647 & 467 & 261 & 621 & 621 \end{array}\right\}, \text{\&c.} \quad . \quad . \quad . \quad \text{(A)}$$

where every first vertical row is one of the fundamental triads. We can thus form twenty-eight triplets of triads, and exhaust 3 . 7 . 4 of the 21 . 10 couplets possible with the twenty-one triads 157, 571, &c.

We can next form a quadruplet upon each of the twenty-one triads thus:—

on (157) 517	on (524) 254	on (563) 653
126	517	517
751	452	365
134	563	524

In the first of these, 157, 126, 134 are three triads with

the same capital, and 157, 571, 715 are three which have the same figures; and so on with the rest.

The 21 quadruplets thus formed exhaust the 21.6 duads not found in the 28 triplets, so that we have once and once only employed the duads possible with the 21 triads in these 21 quadruplets and 28 triplets.

The whole of the triplets and quadruplets are thus written:—

157	157	517	517	571	571	751	751	175	175	715	
327	237	327	237	431	341	431	341	365	635	365	
467	647	647	467	261	621	621	261	425	245	245	
715	372	372	732	732	273	273					
425	452	612	542	612	653	563					(A)
635	162	542	162	432	143	413					
723	723	674	674	764	764	746		746	476	476	
653	563	254	314	254	314	126		216	126	216	
413	143	134	524	314	254	536		356	356	536	

These triplets may be denoted thus: $(134)_7$, $(126)_7$, &c.

(157)	(327)	(467)	(237)	(647)	(431)	(261)	
517	237	674	372	476	314	612	
126	341	413	261	621	467	254	
751	723	746	723	764	143	126	
134	365	425	254	635	452	273	
(126)	(134)	(254)	(341)	(356)	(517)	(715)	
261	341	542	413	563	175	157	
157	126	237	327	327	524	723	(B)
612	413	425	134	635	751	571	
134	157	261	356	341	536	746	
(723)	(746)	(612)	(653)	(452)	(536)	(542)	
237	467	126	536	524	365	425	
715	723	647	612	413	524	536	
372	674	261	365	245	653	254	
746	715	653	647	476	517	517	

If we consider these 28 triplets and 24 quadruplets to be systems of didymous factors of as many substitutions of the third and fourth orders, $a_3 b_3$, &c., $A_4 B_4$, &c., we may de-

fine (157), which determines the first quadruplet, as permutable with each of its four substitutions; *i. e.* $(157)=A_4^2$, $(327)=B_4^2$, &c.

We have before us every duad of the 21 triads; we have next to give an account of all the triplets possible. The condition that a triplet RPQ of these triads should be reducible to a duad have been already given. Suppose P=157, Q=327, and that

$$R.P.Q=R157.327=R.327.467=R467.157$$

is irreducible. RP cannot have a common capital, nor RQ, otherwise they would be permutable; nor can R be any permutation of 157, 327, or 467, for the same reason. Then R can only be a permutation of 261, or 635, or 524, which has not the capital 1, 3, or 4. Whatever it may be, it cannot have a small figure in common with both 57 and 27; and RP, in one of the above forms, will be a consecutive pair in one of the quadruplets (B). Let this be RPST; then RP=TR, and RPQ=TRQ, where R has neither of the small figures of Q, so that RQ is a consecutive pair in some one of the quadruplets (B); and RQ is of the fourth order. We have thus proof that every irreducible triplet RPQ either has PQ of the fourth order, or can be written as TRQ where RQ is of the fourth order. We have then occasion only to consider the irreducible triplets of the form D.517.126. How many values can D have, so that this shall be irreducible? The capital of D must be one of 76432. {517.126}=(157) has 751 permutable with 723, therefore D is not 723: {746.517}=(715) has 157 permutable with 126, therefore D is not 746; and D cannot be 715 permutable with 517. Therefore D has not the capital 7. But {635.517}=(536) has no permutable of 126, nor has {517.126}=(157) a permutable of 635; nor has {635.126}=(612) a permutable of 126.517.126=751. Therefore 635.517.126 is irreducible. Next, {647.517}=$(536)_7$ has no permutable of

126; nor has $\{517 . 126\} = (157)$ a permutable of (647); neither has $\{647 . 126\} = (612)$ a permutable of

$$126 . 517 . 126 = 751.$$

Therefore 647 . 517 . 126 is irreducible.

Precisely in the same way it is proved that all the eight following values of D,

$$635, \quad 647, \quad 425, \quad 467, \quad 372, \quad 356, \quad 273, \quad 245,$$

render D . 517 . 126 irreducible. And there are no more values, because we cannot have D=612 permutable with 126, nor D=431 permutable with 134 in (157), nor D=314 for the same reason, nor D=216 permutable with 126.

We can thus demonstrate, what is indeed sufficiently evident from symmetry, that there are eight irreducible triplets D . RQ, whatever substitution of the fourth order RQ may be

We cannot proceed further without a closer definition of our 21 triads. We define 157, 126, 134 as the mutually permutables

$$1643527, \quad 1243765, \quad 1634725,$$

all of the same form and of the second order. In like manner

$$517 = 1462537, \quad 524 = 7264351, \quad 536 = 7432561,$$

which mutually determine each other,

$$517 . 126 = 1462537 . 1243765 = 1426735,$$

of which the circular factors are 1, 75, 2463. None of the eight values has three figures of the circle 2463, and none has 1. Therefore each of them exchanges 1 for one of 2463, and either 7 or 5 for another of 2463; that is, each first makes the three circles of 1426735 into two circles of five and two, and then unites these two into one of seven. Hence it appears that the eight irreducible triplets are all substitutions of the seventh order.

I know not whether the following theorem has ever been

formally enunciated. It is of considerable importance in the theory of substitutions, and very easily established.

Theorem. *The transposition of two letters in any circular factor always fractures that circle into two: the transposition of two letters of two circular factors always unites those circles into one.*

We have forty-two different substitutions of the fourth order, each of which has four forms, $ab=bc=cd=da$, in terms of the didymous factors $abcd$; and on each (PQ) of these four we form eight irreducible triplets D . PQ, giving in all 8 . 42 . 4 irreducible triplets.

Let D . PQ and D′. PQ be two of the eight irreducibles made on PQ: we cannot have

$$D'.PQ=(D.PQ)^2=DPQDPQ$$

unless

$$D'=DPQD=D.(PQ)D^{-1},$$

which is impossible, because D′ is of the second order, and DPQD is of the order of PQ, that is, of the fourth. Neither can we have

$$D'PQ=(DPQD)^3=DPQDPQDPQ$$

unless

$$D'=DPQDPQD,$$

whence

$$DD'D=PQDPQ,$$

and

$$1=(PQDPQ)^2, =(DD'D)^2=D^2;$$

which is false, because PQ is not $=(PQ)^{-1}$, PQ being of the fourth order.

It is thus shown that no one of the eight irreducibles made on PQ can be a power of another of them. Hence there are not less than $8r$ substitutions of the seventh order, no one of which is a power of another; that is, there are $8.6r$ different substitutions of the seventh order; and as each has four values PQ for the same D, the number 8 . 7 . 6 . 4 of triplets irreducible must be divisible by $8.6.4r$, whence $r=1$, or $r=7$.

The entire non-modular group consists of 2 . 28 substitutions of the third order, 2 . 21 of the fourth, 21 of the second, and 8 . 6*r* of the seventh, which gives

$$8r.6_7+28.2_3+21.2_4+21.1_2+1=7.6.4+8(r-1),$$

which is no divisor of 7 . 6 . 5 . 4 . 3 . 2, if $r=7$. Wherefore $r=1$.

The 21 substitutions of the second order are

157=1643527,	261=1275463,	372=4231657,	
126=1243765,	237=6235417,	314=1734652,	
134=1634725,	245=6274513,	365=4731562,	
413=1534276,	517=1462537,	612=1257364,	715=1326547,
425=3214576,	524=7264531,	635=2137564,	723=5236147,
467=3514267,	563=7432561,	647=2154367,	746=5324167.

Those of the seventh order are the powers of

4231657 . 1426735=4125736,	4731562 . 1426735=4176235,
6235417 . 1426735=6521734,	2137564 . 1426735=2716435,
3214576 . 1426735=3427615,	3514267 . 1426735=3456712,
6274513 . 1426735=6421375,	2154367 . 1426735=2416753.

The 28 triplets and the 21 quadruplets give those of the third and fourth orders. And thus the entire group of 7 . 6 . 4 is readily constructed.

I have shown in the memoir above quoted, art. 93, that two groups of 7 . 6 . 4 can be constructed to contain the powers of 3456712 last but one above written. I have no doubt that the same thing is proveable by beginning this investigation with the only other system of triads that can be made to exhaust the duads in 7. We ought to find that the powers of 3456712 are also part of the second group of 7 . 6 . 4 so constructed.

This group is maximum, *i. e.* it has no derived derangements, as may be proved by finding the number of its equivalents, which is 30.

As I have shown in the abstract of this paper printed in the 'Proceedings' of the Literary and Philosophical Society of Manchester, April 29, 1862, that we can demonstrate

and construct the non-modular group of 11 . 10 . 6 which has 120 substitutions of the eleventh order, 264 of the fifth, 110 of the sixth, and 55 of the second order, by a method similar to that above given for the group of 7 . 6 . 4, there is no necessity for here amplifying what I have there written, after what has been said on the group of 7 . 6 . 4. I hope to return before long to this subject.

XV.—*Note on Differential Resolvents.*

By William Spottiswoode, M.A., F.R.S.

Communicated by the Rev. Robert Harley, F.R.A.S.

Read November 4, 1862.

The following seems the readiest method of finding the differential resolvent of a given algebraic equation, the coefficients of which are functions of a single parameter. Although exemplified here only in the cases of quadratics and cubics, it is directly applicable to all degrees.

Beginning with the quadratic

$$(a, b, c\between x, 1)^2=0, \quad . \quad . \quad . \quad . \quad . \quad (1)$$

and indicating differentiation with respect to the parameter by accents, we have

$$2(a, b\between x, 1)x'+(a', b', c'\between x, 1)^2=0, \quad . \quad . \quad . \quad (2)$$

from which we may form the following system:—

$$\left.\begin{array}{llllll} -(a, b\between x, 1)(-2x') & +a'x^2 & +2b'x & +c' & =0, \\ -(a, b\between x, 1)0 & +ax^2 & +2bx & +c & =0, \\ -(a, b\between x, 1)1 & \quad . & +ax & +b & =0, \\ -(a, b\between x, 1)x & +ax^2 & +bx & \quad . & =0, \end{array}\right\} \quad . \quad . \quad (3)$$

whence

$$\begin{vmatrix} -2x' & a' & 2b' & c' \\ . & a & 2b & c \\ 1 & . & a & b \\ x & a & b & . \end{vmatrix} = 0, \quad . \quad . \quad . \quad . \quad (4)$$

the differential resolvent required. The developed form is

$$\left.\begin{array}{l} 2a(ac-b^2)x' - \{a'(2b^2-ac) - 2b'ab + c'a^2\}x \\ \quad -a'bc + 2b'ca - c'ab = 0. \end{array}\right\} \quad . \quad . \quad (5)$$

Proceeding to the cubic

$$(a, b, c, d \between x, 1)^3 = 0$$

and differentiating, we may form the system

$$\left.\begin{array}{llllllll} -(a, b, c \between x, 1)^2(-3x') + & . & a'x^3 & +3b'x^2 & +3c'x & +d' & =0, \\ -(a, b, c \between x, 1)^2 0 & + \quad . & ax^3 & +3bx^2 & +3cx & +d & =0, \\ -(a, b, c \between x, 1)^2 0 & +ax^4 & +3bx^3 & +3cx^2 & +dx & . & =0, \\ -(a, b, c \between x, 1)^2 1 & + \quad . & . & ax^2 & +2bx & +c & =0, \\ -(a, b, c \between x, 1)^2 x & + \quad . & ax^3 & +2bx^2 & +cx & . & =0, \\ -(a, b, c \between x, 1)^2 x^2 & +ax^4 & +2bx^3 & +cx^2 & . & . & =0, \end{array}\right\} (6)$$

whence

$$\begin{vmatrix} -3x' & . & a' & 3b' & 3c' & d' \\ . & . & a & 3b & 3c & d \\ . & a & 3b & 3c & d & . \\ 1 & . & . & a & 2b & c \\ x & . & a & 2b & c & . \\ x^2 & a & 2b & c & . & . \end{vmatrix} = 0 \quad . \quad . \quad . \quad (7)$$

Writing this in the form

$$\mathrm{A}x' = \mathrm{E}x^2 + \mathrm{F}x + \mathrm{G} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

and differentiating, we may form the system

$$\left.\begin{array}{l} -Ax''+2Exx'+(F-A')x' + \;.\; +E'x^2+F'x+G'=0, \\ \;.\; \quad -Axx' - \quad . \quad +Ex^3+Fx^2+Gx \;.\; =0, \\ \;.\; \qquad . \qquad Ax' + \;.\; +Ex^2+Fx+G=0, \\ \;.\; \qquad . \qquad . \quad ax^3+3bx^2+3cx+d=0, \end{array}\right\}(9)$$

or, as it may be more concisely written,

$$\left|\begin{array}{ccccccc} x'' & xx' & x' & x^3 & x^2 & x & 1 \\ \hline -A & 2E & F-A' & . & E' & F' & G' \\ . & -A & . & E & F & G & . \\ . & . & -A & . & E & F & G \\ . & . & . & a & 3b & 3c & d \end{array}\right| =0, \quad . \; (10)$$

from which we may eliminate linearly any three of the quantities x'', xx', x', x^3, x^2, x, 1. If we eliminate xx', x^3, x^2, we have the differential resolvent, viz.

$$\left|\begin{array}{cccr} 2E & . & E' & -Ax''+(F-A')x'+F'x+G' \\ -A & E & F & Gx \\ . & . & E & -Ax'+Fx+G \\ . & a & 3b & 3cx+d \end{array}\right| =0 \,; \; . \; (11)$$

the developed form of which is

$$\left.\begin{array}{l} A^2Eax'' \\ +A\{a(A'E-AE')-3aEF+6bE^2\}x' \\ +\;\{aA(E'F-EF')+2aE(F^2-EG)-6E^2(bF-cE)\}x \\ +\;\{aA(E'G-EG')+2aEFG \qquad -2E^2(3bG-dE)\}=0. \end{array}\right\}(12)$$

This agrees with a result communicated to me by Mr. Harley.

It seems possible to exhibit the resolvent as a single determinant; but as this is of the 16th degree, and does not (at least so far as I have found) exhibit the discriminant as a factor, I have set it aside as too unwieldy for use.

The developed values of A, E, F, G are as follow :—

$$\frac{\mathrm{A}}{a}=-3b^2c^2+4b^3d-6abcd+4ac^3+a^2d^2.$$

$$\begin{aligned}\frac{3\mathrm{E}}{a}=&a'(4b^2d-3bc^2-acd)\\&-6b'a(bd-c^2)\\&-3c'a(bc-ad)\\&-2d'a(ac-b^2).\end{aligned}$$

$$\begin{aligned}\frac{3\mathrm{F}}{a}=&a'(7bcd-ad^2-6c^3)\\&+3b'(3bc^2-2b^2d-acd)\\&+3c'(-3b^2c+2ac^2+abd)\\&+d'(-7abc+a^2d+6b^3).\end{aligned}$$

$$\begin{aligned}\frac{3\mathrm{G}}{a}=&2a'd(bd-c^2)\\&+3b'd(bc-ad)\\&+6c'd(ac-b^2)\\&+d'(-4ac^2+3b^2c+abd).\end{aligned}$$

Now Mr. Harley has shown in a recent letter, that the coefficients of x and 1 in (12) are divisible by A (the discriminant of the original cubic); the result, after various reductions, is

$$\begin{aligned}&\mathrm{AE}x''\\&+\left\{\mathrm{A'E}-\mathrm{AE'}-3\mathrm{EF}+6\frac{b}{a}\mathrm{E}^2\right\}x'\\&+\frac{1}{9}\{9(\mathrm{E'F}-\mathrm{EF'})\\&\qquad-2\mathrm{E}[a'^2(-ad^2-9c^3+9bcd)\\&\qquad+9b'^2a(2bd-3c^2)\\&\qquad-9c'^2a^2c\\&\qquad-d'^2a^3\\&\qquad+3a'b'(-6b^2d+9bc^2-acd)\end{aligned}$$

$$
\begin{aligned}
&+3a'c'(6ac^2-9b^2c+abd)\\
&+2a'd'(a^2d+9b^3-9abc)\\
&-9b'c'a(ad-3bc)\\
&+6b'd'a(2ac-3b^2)\\
&+6c'd'a^2b]\}x
\end{aligned}
$$

$$
\begin{aligned}
+\frac{1}{9}\{9(\mathrm{E'G}-\mathrm{EG'})\\
&+2\mathrm{E}[a'^2d(3c^2-2bd)\\
&+9b'^2acd\\
&+9c'^2a^2d\\
&+a'^2a^2b\\
&+3a'b'ad^2\\
&+6a'c'd(3b^2-2ac)\\
&+a'd'(6ac^2-9b^2c+abd)\\
&+18b'c'abd\\
&+3b'd'a(-ad+3bc)\\
&-6c'd'a^2c]\},
\end{aligned}
$$

or, as it may be more concisely written,

$$
\mathrm{A\,E}\,x''
+\left\{\begin{vmatrix}\mathrm{A} & \mathrm{A'}\\ \mathrm{E} & \mathrm{E'}\end{vmatrix}+\frac{3\mathrm{E}}{a}(3b\mathrm{E}-a\mathrm{F})\right\}x'
$$

$$
+\begin{vmatrix} . & \mathrm{E} & \mathrm{E'}\\ 1 & \mathrm{F} & \mathrm{F'}\\ x & \mathrm{G} & \mathrm{G'}\end{vmatrix}
+2\mathrm{E}\begin{vmatrix}
. & . & a' & 3b' & 3c' & d'\\
. & a' & 3b' & 3c' & d' & .\\
. & . & a & 3b & 3c & d\\
. & a & 3b & 3c & d & .\\
1 & . & . & a & 2b & c\\
x & . & a & 2b & c & .
\end{vmatrix}=0.
$$

As regards ulterior developments, I have at present only to add that it may be readily shown by the method of compound determinants that the part of

$$
\begin{vmatrix} . & \mathrm{E} & \mathrm{E'}\\ 1 & \mathrm{F} & \mathrm{F'}\\ x & \mathrm{G} & \mathrm{G'}\end{vmatrix}
$$

involving a'', $3b''$, $3c''$, d'', is equal to

$$A+\left|\begin{array}{ccccc} . & a'' & 3b'' & 3c'' & d'' \\ . & a' & 3b' & 3c' & d' \\ . & a & 3b & 3c & d \\ 1 & . & a & 2b & c \\ x & a & 2b & c & . \end{array}\right|$$

XVI.—*On a Certain Class of Linear Differential Equations.* By the Rev. Robert Harley, F.R.A.S., Corresponding Member of the Literary and Philosophical Society of Manchester.

Read November 4, 1862.

In the Philosophical Magazine for May of last year Mr. Cockle showed, in a paper entitled "On Transcendental and Algebraic Solution," that from any algebraic equation of the degree n, whereof the coefficients are functions of a variable, there may be derived a linear differential equation of the order $n-1$, which will be satisfied by any one of the roots of the given algebraic equation. The connexion of this theorem with a certain general process for the solution of algebraic equations, led me to consider its application to the form

$$y^n-ny+(n-1)x=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (I)$$

to which it is known that any equation of the nth degree, when n is not greater than 5, can, by the aid of equations of inferior degrees, be reduced.

In the course of my investigations I was conducted to the conclusion that for all integral values of n between the limits

$$n=2, n=5,$$

both inclusive, the linear differential equation, or, as it is proposed to call it, the "differential resolvent," is of the form

$$\left\{a_0+a_1x\frac{d}{dx}+a_2x^2\left(\frac{d}{dx}\right)^2 \ldots +a_{n-1}x^{n-1}\left(\frac{d}{dx}\right)^{n-1}\right\}y$$
$$=a_{n-1}\left(\frac{d}{dx}\right)^{n-1}y,$$

and I completely determined the constants $a_0, a_1, \ldots a_{n-1}$ for all the cases up to and including $n=5$.

I found, moreover, that this result, in itself sufficiently remarkable, might be put under a still more simple and striking form by following a process of transformation proposed by Prof. Boole in his "Memoir on a General Method in Analysis," which appeared in the Philosophical Transactions for 1844, Part II. I found in fact that, writing ϵ^θ for x, and D for $x\frac{d}{dx}$ or $\frac{d}{d\theta}$, the differential resolvent of the trinomial equation (I) may be made to take the form

$$\mathrm{D}(\mathrm{D}-1)(\mathrm{D}-2)\ldots(\mathrm{D}-n+2)y$$
$$-\left(\mathrm{D}-\frac{2n-1}{n}\right)\left(\mathrm{D}-\frac{3n-2}{n}\right)\left(\mathrm{D}-\frac{4n-3}{n}\right)$$
$$\ldots\left(\mathrm{D}-\frac{n^2-n+1}{n}\right)\epsilon^{(n-1)\theta}y=0, \ldots \text{(A)}$$

the case $n=2$ being an exception. In this exceptional case the sum of the roots (Σy) is not, as in the other cases, equal to zero, and the differential resolvent must therefore contain a term independent of y. This term written on the dexter $=\frac{1}{2}\epsilon^\theta$, and the terms on the sinister follow the law above indicated.

Using the ordinary factorial notation, that is to say, representing

$$(u)(u-1)(u-2)\ldots(u-r+1)$$

by $[u]^r$, the form (A) may be written

$$n^{n-1}\left[x\frac{d}{dx}\right]^{n-1}y-(n-1)^{n-1}\left[\frac{n}{n-1}x\frac{d}{dx}-\frac{2n-1}{n-1}\right]^{n-1}x^{n-1}y=0\ldots\text{(B)}$$

In the 'Proceedings' of this Society (vol. ii. pp. 181–184) for the 4th of February last, I gave, without the details of calculation, the several differential resolvents for the successive cases $n=2, 3, 4, 5$; and these results Mr. Rawson of Portsmouth has kindly verified. I gave also in the same paper, the Boolian (symbolical) form of the resolvent for the biquadratic; and this seems to have suggested to Mr. Cayley an investigation in which he showed, by the aid of Lagrange's theorem, that the equation (B) holds for all values of n. I had the honour of communicating Mr. Cayley's investigation to the Society on the ensuing 18th of February; and an abstract of it appeared at p. 193, vol. ii. of the 'Proceedings.' The paper itself is printed in this volume of Memoirs, at p. 111. Before receiving Mr. Cayley's remarkable analysis I had calculated, and I believe I had also communicated to Mr. Cockle, the Boolian forms of the resolvents for the cases $n=2$ to $n=5$, both inclusive; and these suggested to me the general form (A). (See 'Proceedings,' vol. ii. pp. 199–201, and pp. 237–241.)

The singular simplicity of these results for the trinomial algebraic equation (I) had an effect in inducing me to consider the corresponding form

$$y^n-ny^{n-1}+(n-1)x=0 \quad . \quad . \quad . \quad . \quad . \quad \text{(II)}$$

to which also any algebraic equation of the nth degree, n being not greater than 5, can, as Mr. Jerrard has shown, be reduced by means of equations of inferior degrees; and by induction I was led to the following general expression for its resolvent, viz.

$$n^{n-1}[(n-1)\mathrm{D}]^{n-1}y-(n-1)(n\mathrm{D}-n-1)[n\mathrm{D}-2]^{n-2}\epsilon^{\theta}y$$
$$=[n-1]^{n-1}\epsilon^{\theta}, \quad . \quad . \quad . \quad . \quad . \quad \text{(C)}$$

or, what is the same thing,

$$n^{n-1}\left[(n-1)x\frac{d}{dx}\right]^{n-1}y-(n-1)\left(nx\frac{d}{dx}-n-1\right)$$
$$\left[nx\frac{d}{dx}-2\right]^{n-2}xy=[n-1]^{n-1}x. \quad . \quad . \quad . \quad \text{(D)}$$

The deduction of the most general results from the canonical resolvent is properly a subject for separate discussion, and I hope to treat of it in a future memoir; but there is a particular case so marked in character, and lying so near at hand, that I am induced to present it here. The differential resolvents for

$$y^n-nay+(n-1)bx=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(III)}$$

and

$$y^n-nay^{n-1}+(n-1)bx=0 \quad . \quad . \quad . \quad . \quad . \quad \text{(IV)}$$

where a and b are any constants, are

$$a^n n^{n-1}[\mathrm{D}]^{n-1}y-b^{n-1}(n-1)^{n-1}\left[\frac{n\mathrm{D}-(2n-1)}{n-1}\right]^{(n-1)}$$
$$\epsilon^{(n-1)\theta}y=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(E)}$$

and

$$a^n n^{n-1}[(n-1)\mathrm{D}]^{n-1}y-b(n\mathrm{D}-n-1)[n\mathrm{D}-2]^{n-2}\epsilon^\theta y$$
$$=[n-1]^{n-1}ab\epsilon^\theta \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(F)}$$

respectively. For by simply writing

$$\frac{bx}{a^{\frac{n}{n-1}}} \text{ for } x$$

and

$$\frac{y}{a^{\frac{1}{n-1}}} \text{ for } y,$$

the equation (I) is transformed into the equation (III); and these substitutions being made in the differential resolvent (A), the symbol D remaining for these substitu-

tions unchanged, we pass at once to the form (E). In like manner writing

$$\frac{bx}{a^n} \text{ and } \frac{y}{a}$$

for x and y respectively, the equation (II) takes the form (IV); and making these substitutions in (C), D remaining as before unchanged, we obtain the differential resolvent of (IV), viz. (F).

The particular cases on which the foregoing inductions were founded are as follows:—

1. I begin with the form (I), in which I assign to n the successive values 2, 3, 4, 5. There result the equations

$$\begin{aligned} y^2-2y+x&=0, \\ y^3-3y+2x&=0, \\ y^4-4y+3x&=0, \\ y^5-5y+4x&=0, \end{aligned}$$

whose resolvents I now proceed to calculate.

2. In the case of the quadratic, a single differentiation gives

$$\frac{dy}{dx}=-\frac{1}{2}\cdot\frac{1}{y-1}=-\frac{1}{2}\cdot\frac{1}{1-x}(y-1),$$

or

$$2(1-x)\frac{dy}{dx}+y=1,$$

the resolvent required.

3. In the case of the cubic we have, by successive differentiations and reductions,

$$3(1-x^2)\frac{dy}{dx}=-(y^2+xy-2),$$

$$3^2(1-x^2)^2\frac{d^2y}{dx^2}=-\{3xy^2+(1+2x^2)y-6x\}.$$

Combining these equations so as to eliminate y^2, and sim-

plifying the result, we obtain

$$3^2(1-x^2)\frac{d^2y}{dx^2}-3^2x\frac{dy}{dx}+y=0,$$

the resolvent required.

4. In the case of the biquadratic, the same process gives the equations

$$4\,(1-x^3)\frac{dy}{dx}=-(y^3+xy^2+x^2y-3),$$

$$4^2(1-x^3)^2\frac{d^2y}{dx^2}=-\{6x^2y^3+(1+5x^3)y^2$$
$$+3(1+x^3)xy-18x^2\},$$

$$4^3(1-x^3)^3\frac{d^3y}{dx^3}=-\{(43+65x^3)xy^3$$
$$+(61+47x^3)x^2y^2$$
$$+(10+77x^3+21x^6)y$$
$$-3(43+65x^3)x\},$$

which, combined so as to eliminate y^3 and y^2, give

$$2^5(1-x^3)\frac{d^3y}{dx^3}-2^4\cdot 3^2x^2\frac{d^2y}{dx^2}$$
$$-2\cdot 43x\frac{dy}{dx}+5y=0,$$

the differential resolvent for the biquadratic.

5. For the quintic we have in like manner

$$5\,(1-x^4)\frac{dy}{dx}=-(y^4+xy^3+x^2y^2+x^3y-4),$$

$$5^2(1-x^4)^2\frac{d^2y}{dx^2}=-\{10x^3y^4+(1+9x^4)y^3$$
$$+(3+7x^4)xy^2$$
$$+2(3+2x^4)x^2y-40x^3\},$$

$$5^3(1-x^4)^3\frac{d^3y}{dx^3}=-\{15(9+11x^4)x^2y^4$$
$$+15(11+9x^4)x^3y^3$$
$$+3(4+67x^4+29x^8)y^2$$
$$+3(17+71x^4+12x^8)xy$$
$$-60(9+11x^4)x^2\},$$

$$5^4(1-x^4)^4\frac{d^4y}{dx^4}=-\{75(17+134x^4+49x^8)xy^4$$
$$+60(31+173x^4+46x^8)x^2y^3$$
$$+30(121+328x^4+51x^8)x^3y^2$$
$$+3(77+2214x^4+2541x^8+168x^{12})y$$
$$-300(17+134x^4+49x^8)x\}.$$

And combining as before, so as to eliminate all powers of y higher than the first, we find

$$5^4(1-x^4)\frac{d^4y}{dx^4}-2\,.\,5^5x^3\frac{d^3y}{dx^3}-3^2\,.\,5^3\,.\,13x^2\frac{d^2y}{dx^2}$$
$$-3\,.\,5^3\,.\,17x\frac{dy}{dx}+3\,.\,7\,.\,11y=0,$$

the differential resolvent for the quintic.

6. If now we collect these several resolvents and apply to them Dr. Boole's process for passing from the ordinary to the symbolical form of a differential equation, we find that

For the quadratic, the resolvent is

$$Dy-\left(D-\frac{3}{2}\right)\epsilon^\theta y=\frac{1}{2}\epsilon^\theta.$$

For the cubic, it is

$$D(D-1)y-\left(D-\frac{5}{3}\right)\left(D-\frac{7}{3}\right)\epsilon^{2\theta}y=0,$$

or, which is the same thing,

$$[D]^2y-\left(\frac{2}{3}\right)^2\left[\frac{3D-5}{2}\right]^2\epsilon^{2\theta}y=0.$$

For the biquadratic, it is

$$D(D-1)(D-2)y-\left(D-\frac{7}{4}\right)\left(D-\frac{10}{4}\right)\left(D-\frac{13}{4}\right)\epsilon^{3\theta}y=0,$$

or, what is the same thing,

$$[D]^3y-\left(\frac{3}{4}\right)^3\left[\frac{4D-7}{3}\right]^3\epsilon^{3\theta}y=0.$$

For the quintic, it is

$$D(D-1)(D-2)(D-3)y$$
$$-\left(D-\frac{9}{5}\right)\left(D-\frac{13}{5}\right)\left(D-\frac{17}{5}\right)\left(D-\frac{21}{5}\right)\epsilon^{4\theta}y=0,$$

or, what is the same thing,

$$[D]^4y-\left(\frac{4}{5}\right)^4\left[\frac{5D-9}{4}\right]^4\epsilon^{4\theta}y=0;$$

whence the general form (A) or (B) assigned to the resolvent of the algebraic equation (I).

7. We take now the form (II), and give to n the successive values 2, 3, 4, 5. There result the equations

$$y^2-2y+x=0,$$
$$y^3-3y^2+2x=0,$$
$$y^4-4y^3+3x=0,$$
$$y^5-5y^4+4x=0.$$

We have already (Art. 2) calculated the resolvent of the first of these equations; it only remains to calculate the resolvents of the last three.

8. For the cubic, we have

$$3x(2-x)\frac{dy}{dx}=-y^2+(3-x)y+x,$$

$$3^2x^2(2-x)^2\frac{d^2y}{dx^2}=3(1-x)y^2$$
$$-(3^2-2\,.\,5x+2x^2)y$$
$$-(1-2x)x;$$

whence the differential resolvent

$$3^2x(2-x)\frac{d^2y}{dx^2}+3^2(1-x)\frac{dy}{dx}+y=1.$$

9. For the biquadratic, we have

$$4x(3^2-x)\frac{dy}{dx}=-y^3+y^2+(2^2\,.\,3-x)y+x,$$

$$4^2x^2(3^2-x)^2\frac{d^2y}{dx^2}=2(3^2-2x)y+2xy^2$$
$$-(2^5\,.\,3^2-5\,.\,13x+3x^2)y$$
$$-3(3-x)x,$$

$$4^3x^3(3^2-x)^3\frac{d^3y}{dx^3}=-(2^4\,.\,3^2\,.\,7-7\,.\,43x+3\,.\,11x^2)y^3$$
$$-(2^5\,.\,3^2-5\,.\,19x-5x^2)y^2$$
$$+(2^7\,.\,3^3\,.\,5-2\,.\,3\,.\,953x+647x^2-3\,.\,7x^3)y$$
$$+(2\,.\,3^2\,.\,29-139x+3\,.\,7x^2)x\,;$$

whence the resolvent

$$2^5x^2(3^2-x)\frac{d^3y}{dx^3}+2^4x(2^2\,.\,3^2-7x)\frac{d^2y}{dx^2}$$
$$+2(2^5-23x)\frac{dy}{dx}+y=1.$$

10. For the quintic, we have

$$5x(4^3-x)\frac{dy}{dx}=-y^4+y^3+2^2y^2+(2^4\,.\,5-x)y+x,$$

$$5^2x^2(4^3-x)^2\frac{d^2y}{dx^2}=5(2^5-x)y^4+2(2^4+x)y^3$$
$$-5(2^6-3x)y^2-2^2(2^6\,.\,3\,.\,5^2$$
$$-149x+x^2)y-2(2^3\,.\,3-x)x,$$

$$5^3x^3(4^3-x)^3\frac{d^3y}{dx^3}=-3(2^8\,.\,107-2^2\,.\,3^2\,.\,31x+17x^2)y^4$$
$$-2^2\,.\,3\,.\,5(2^6\,.\,3^2-29x)y^3$$
$$+3\,.\,5(2^{10}\,.\,5-2^4\,.\,13x+7x^2)y^2$$
$$+2\,.\,3(2^{11}\,.\,5^3\,.\,7-2\,.\,2593x$$
$$+3\,.\,431x^2-2\,.\,3x^3)y$$
$$+2^2\,.\,3(2^6\,.\,67-3\,.\,53x+3x^2)x,$$

$$5^4x^4(4^3-x)^4\frac{d^4y}{dx^4}=2\,.\,3\{(2^{12}\,.\,5\,.\,563-2^{10}\,.\,5^4x$$
$$+5\,.\,2601x^2-2\,.\,5\,.\,13x^3)y^4$$
$$+(2^{12}\,.\,5^2\,.\,59-2^8\,.\,5\,.\,323x$$
$$+5\,.\,7\,.\,287x^2-2^3\,.\,5x^3)y^3$$
$$-(2^{13}\,.\,3\,.\,5^3-2^8\,.\,5\,.\,31x$$
$$-2\,.\,3\,.\,5\,.\,7x^2-5\,.\,31x^3)y^2$$

$$-(2^{15}\,.\,5^4\,.\,7\,.\,11-2^{10}\,.\,3^3\,.\,3527x$$
$$+2^3\,.\,282949x^2-2\,.\,11789x^3$$
$$+2^2\,.\,3\,.\,7x^4)y$$
$$-(2^{12}\,.\,1801-2^8\,.\,1507x$$
$$+7103x^2-2^2\,.\,3\,.\,7x^3)x\};$$

whence the resolvent

$$5^4x^3(4^3-x)\frac{d^4y}{dx^4}+5^4x^2(2^5\,.\,3^2-7x)\frac{d^3y}{dx^3}$$
$$+3\,.\,5^3\,.\,17x(2^2\,.\,5-x)\frac{d^2y}{dx^2}+2\,.\,3\,.\,5^2(5^2-3^2x)\frac{dy}{dx}$$
$$+2\,.\,3y=2\,.\,3.$$

11. Collecting results and passing, as before, from the ordinary to the Boolian or symbolical form, we find that

For the quadratic, the resolvent is

$$2\mathrm{D}y-(2\mathrm{D}-3)\epsilon^\theta y=\epsilon^\theta.$$

For the cubic, it is

$$3^2[2\mathrm{D}]^2y-2(3\mathrm{D}-4)(3\mathrm{D}-2)\epsilon^\theta y=[2]^2\epsilon^\theta.$$

For the biquadratic, it is

$$4^3[3\mathrm{D}]^3y-3(4\mathrm{D}-5)[4\mathrm{D}-2]^2\epsilon^\theta y=[3]^3\epsilon^\theta.$$

For the quintic, it is

$$5^4[4\mathrm{D}]^4y-4(5\mathrm{D}-6)[5\mathrm{D}-2]^3\epsilon^\theta y=[4]^4\epsilon^\theta.$$

From these four cases the general form for the resolvent of the equation (II) is sufficiently obvious; but I have thought it well to test that form by the case $n=6$, or, what is the same thing, by the sextic equation

$$y^6-6y^5+5x=0,$$

for which the general form (C) gives

$$6^5[5\mathrm{D}]^5y-5(6\mathrm{D}-7)[6\mathrm{D}-2]^4\epsilon^\theta y=[5]^5\epsilon_\theta.$$

12. Returning by the usual method from the symbolical

to the ordinary form of a differential equation, we find

$$2^3 \cdot 3^4 x^4 (5^4 - x) \frac{d^5y}{dx^5} + 2^2 \cdot 3^4 (2^4 \cdot 5^4 - 23x) \frac{d^4y}{dx^4}$$

$$+ 2 \cdot 3^2 (2^5 \cdot 3^4 \cdot 5^3 - 1249x) \frac{d^3y}{dx^3}$$

$$+ 3^2 (2^6 \cdot 3^3 \cdot 5^3 - 11 \cdot 181x) \frac{d^2y}{dx^2}$$

$$+ 2(2^5 \cdot 3^5 - 1051x) \frac{dy}{dx} + 2y = 2.$$

The accuracy of this equation may be tested as follows :—

From the trinomial sextic we deduce, by successive differentiations,

$$\frac{dy}{dx} = -\frac{5}{6} \cdot \frac{1}{y^4(y-5)},$$

$$\frac{d^2y}{dx^2} = -\frac{5^3}{6^2} \cdot \frac{y-4}{y^9(y-5)^3},$$

$$\frac{d^3y}{dx^3} = -\frac{5^4}{6^3} \cdot \frac{11y^2 - 88y + 180}{y^{14}(y-5)^5},$$

$$\frac{d^4y}{dx^4} = -\frac{5^5}{6^4} \cdot \frac{187y^3 - 2244y^2 + 9140y - 12600}{y^{19}(y-5)^7},$$

$$\frac{d^5y}{dx^5} = -\frac{5^6}{6^5}$$

$$\cdot \frac{4301y^4 - 68816y^3 + 419240y^2 - 1150200y + 1197000}{y^{24}(y-5)^9}.$$

If now we assume $y=1$, then we have $x=1$, and the above values give

$$\frac{dy}{dx} = \frac{5}{2^3 \cdot 3}, \qquad \frac{d^2y}{dx^2} = -\frac{5^3}{2^8 \cdot 3},$$

$$\frac{d^3y}{dx^3} = \frac{5^4 \cdot 103}{2^{13} \cdot 3^3}, \qquad \frac{d^4y}{dx^4} = -\frac{5^5 \cdot 613}{2^{18} \cdot 3^2},$$

$$\frac{d^5y}{dx^5} = \frac{5^8 \cdot 16061}{2^{23} \cdot 5^5},$$

which, substituted in the foregoing resolvent, are found to satisfy it.

I have also verified this resolvent by means of other numerical values of y. These verifications seem to place the accuracy of the sextic resolvent beyond doubt, and to afford additional confirmation of the generality of the form (C) or (D).

13. There is probably some method of passing directly from the differential resolvent (A) to the resolvent (C). The algebraic equations (I) and (II), from which they are derived, are closely related, and may easily be deduced the one from the other.

1°. If in equation (I) we write

$$\left\{\frac{x'}{(n-1)^{n-2}}\right\}^{\frac{1}{n-1}}, \quad \frac{\{(n-1)x'\}^{\frac{1}{n-1}}}{y'},$$

for x, y respectively, it becomes

$$y'^{n}-ny'^{n-1}+(n-1)x'=0,$$

an equation which, the accents being suppressed, is identical with (II).

In this transformation it is to be observed that

$$x\frac{d}{dx}=(n-1)x'\frac{d}{dx'},$$

or

$$\mathrm{D}=(n-1)\mathrm{D}'^{*}.$$

2°. Or, if in equation (I) we write

$$-n', \quad \left(\frac{n'^{n'-1}}{x'}\right)^{\frac{1}{n'+1}}, \quad \left(\frac{1}{n'^{2}x'}\right)^{\frac{1}{n'+1}}y',$$

for n, x, y respectively, it becomes

$$y'^{n'+1}-(n'+1)y'^{n'}+n'x'=0,$$

* More generally: If $x=\phi x'$, then we have

$$x\frac{d}{dx}=\phi u\frac{d}{d\phi u}=\phi u \cdot \frac{1}{\phi' u} \cdot \frac{d}{du}$$

(where the accent denotes differentiation),

whence, if $\frac{\phi u}{\phi' u}=u$, or $\frac{\phi' u}{\phi u}=\frac{1}{u}$, and $\phi u=u^{c}$, then will

$$x\frac{d}{dx}=c^{-1}u\frac{d}{du}.$$

which is of the same form as (II). In this case

$$x\frac{d}{dx}=-(n'+1)x'\frac{d}{dx'},$$

or

$$D=-(n'+1)D'.$$

But simple as these transformations are, they do not enable us to pass, at least directly, from the form (A) to the form (C). The first (1°) leads to

$$n^{n-1}[(n-1)D'+1]^{n-1}\frac{1}{y'}-(n-1)[nD'-1]^{n-1}\frac{\epsilon^{\theta'}}{y'}=0,$$

which is non-linear. And the second (2°) leads to

$$(n'+1)^{n'+1}[-(n'+1)D'+1]^{-(n'+1)}y-n'^{2}$$
$$[-(n'D'+1)]^{-(n'+1)}\epsilon^{\theta'}y'=0,$$

which involves an anomaly. These results will, I think, be considered curious and interesting. At all events, I have thought it worth while to record them here, and I shall probably discuss them at some future time.

14. Every differential resolvent may be regarded under two distinct aspects. It may be considered either, first, as giving in its complete integration the solution of the algebraic equation from which it has been derived, or, secondly, as itself solvable by means of that equation. In fact the two equations, the algebraic and the differential, are *coresolvents*. In the first aspect I have considered the differential equation (A) in a paper entitled "On the Theory of the Transcendental Solution of Algebraic Equations," just published in the 'Quarterly Journal of Pure and Applied Mathematics,' No. 20. I have shown in that paper that every differential resolvent is satisfied, not only by each of the roots, but also by each of the constituents of the roots of the algebraic equation to which it belongs; and that these constituents are in fact the particular integrals of the resolvent equation. In the second aspect,

every differential resolvent of an order higher than the second * gives us, at least when the dexter of its defining equation vanishes†, a new primary form, that is to say, a form not recognized as primary in Professor Boole's theory. And in certain cases in which the dexter does not vanish, a comparatively easy transformation will rid the equation of the dexter term, and the resulting differential equation will be of a new primary form. The same transformation which deprives the algebraic equation of its second term will deprive the differential equation of its dexter term. Thus (*ex. gr.*) if we write $z+1$ in place of y, the equation (II) becomes

$$(z+1)^n-n(z+1)^{n-1}+(n-1)x=0,$$

and the resolvent (C) becomes

$$n^{n-1}[(n-1)\mathrm{D}]^{n-1}(z+1)-(n-1)(n\mathrm{D}-n-1)$$
$$[n\mathrm{D}-2]^{n-2}\epsilon^{\theta}(z+1)=[n-1]^{n-1}\epsilon^{\theta},$$

which, since

$$\begin{aligned}&(n-1)(n\mathrm{D}-n-1)[n\mathrm{D}-2]^{n-2}\epsilon^{\theta}\\&=(n-1)(n-n-1)[n-2]^{n-2}\epsilon^{\theta}\\&=-(n-1)[n-2]^{n-2}\epsilon^{\theta}\\&=-[n-1]^{n-1}\epsilon^{\theta},\end{aligned}$$

may be written simply

$$n^{n-1}[(n-1)\mathrm{D}]^{n-1}z-(n-1)(n\mathrm{D}-n-1)$$
$$[n\mathrm{D}-2]^{n-2}\epsilon^{\theta}z=0.$$

* The resolvent of the trinomial cubic of the form (I) has long been known as solvable. This resolvent is of course of the second order.

† The qualification in the text is necessary, because, of the two equations

$$\phi(\mathrm{D})y=\mathrm{X},\quad \phi(\mathrm{D})y=0,$$

the solution of the former does not in general enable us to obtain that of the latter, though from that of the latter it is well known that the solution of the former can be obtained.

XVII.—*On the Influence of the Earth's Rotation on Winds.*

By Thomas Hopkins, Esq., M.B.M.S.

Read December 16th, 1862.

Great importance has been given by many writers to the effects that are said to be produced by the unequal rotatory velocities of different latitudes on the winds that pass over the surface of the globe. It has been confidently asserted that winds passing from the northern polar regions take with them only the slow rotatory movements of the latitudes from which they are passing, and are *therefore* palpably left behind by the quicker rotating surfaces of more southern parts, converting a north into a north-east wind. A wind from the southern polar regions is said, in like manner, to be converted into a south-east wind. The great trade-winds of the tropics are stated to be, in this way, changed from north and south into north-east and south-east winds. That air, passing from polar to tropical regions, is affected, to some extent, in this way is admitted; but the extent has been greatly exaggerated, apparently in order to support an imaginary theory of the tropical trade-winds.

The atmosphere presses on the surface of the globe with a force equal to about 15 lbs. weight on every square inch, and by that pressure it appears to be made to adhere so strongly to it as to enable the surface readily to take with it the air that is in the part at about its own velocity in every latitude. But if such an effect as that which has been alleged is produced by the cause named *to a palpable extent,* we might reasonably expect to find it in operation over the whole globe when air is in motion upon it; we may therefore inquire whether that is or is not the case.

I have shown, in 'Winds and Storms' *, that the trade-winds are produced by a different cause to that which was supposed to connect them with polar air passing over the surface towards the equator; those winds, however, pass over but a few degrees of latitude, where the rotatory velocities of adjoining parts do not alter so much proportionally within the same range of latitudes as they do nearer to the poles. It is therefore about the latter parts that we might, most confidently, expect to find the truth of the hypothesis of retardation tested. But near the polar regions few winds are found sufficiently continuous to enable us to trace the effects of varying rotatory velocities of the surface upon their direction. Yet we are not without cases in such parts that are suitable to throw light on this subject.

We are informed by navigators that from Victoria-land, in (say) 74° of south latitude, a wind is found blowing towards Tierra del Fuego, in 50° south. But Tierra del Fuego, in 50° of latitude, being 24° further from the south pole, has a more rapid rotation than Victoria-land in 74°; and if it were true that air passing from slower to quicker rotating surfaces was left palpably behind the surface of the part over which it was passing, air flowing from the south over the strip described should constitute an apparent easterly wind moving in a direction the opposite to that of Tierra del Fuego. But as it is asserted by navigators to be not a south-*east*, but a south-*west* wind, bearing ships towards Tierra del Fuego, it must move eastward faster than the surface over which it is passing, although that surface is rotating with successively increasing degrees of velocity. This case, then, affords rather strong evidence of the fallacy of the prevailing theory, that wind passing over a meridian from polar to tropical parts is left behind the rotating surface.

* Published by Longman and Co., London.

In the northern hemisphere, there is no palpable wind that blows southward from a latitude higher than 70°; but one can be traced in the Arctic region, from the mouth of the Mackenzie River, in longitude 134° west, that blows generally in the winter over the whole of the eastern side of North America to the southern point of Florida, in latitude 27° and west longitude 80°. This wind is, in its general direction, north-west. Here, then, we have a case similar in character to that just named, of air passing from the slowly rotating latitude of 70° to the rapidly rotating parallel of 27°; but instead of the air being left behind by the more rapidly moving surface of the earth through the 43° of latitude that it traverses, and thus becoming an apparent easterly wind, it moves faster than the surface through the whole 43° of latitude. In the northern case the air had to pass over rough land, presenting a surface likely to take the air with it; but in the southern it had to pass over water only; yet in neither case was the air left behind by the more rapidly rotating surfaces of the earth. Now these two cases seem to be sufficient to prove that air passing from slower to quicker rotating parts is not liable to be left behind in the way that has been so confidently assumed; and they warrant us in inferring that it may possibly be an assumption that appeared to be found necessary to support a fallacious theory, which seemed to some persons to account for important meteorological phenomena.

It is, however, in accounting for the tropical trade-winds that writers have been the most explicit in explaining the retarding effect on winds of increasing rotatory velocity of the surface of the earth. Kämtz, after stating that air ascended in the tropics, and descended in the polar regions to return to the tropics on the surface, says:—"On this principle we ought to find a north wind in the northern hemisphere, and a south wind in the southern; but these

two directions combine with motion of the earth from west to east, and there results a north-east wind in one hemisphere and a south-east wind in the other. Indeed, as the diameter of the parallel circles continues diminishing in proportion as we recede from the equator, and as all the points situated in the same meridian turn round the axis of the earth in twenty-four hours, it follows that they move with a velocity much greater as they are nearer to the equinoctial line. But the masses of air which flow from the north towards the equator have an acquired velocity much less than that of the region towards which they are directed. They therefore move more slowly than do the points situated near the equator, and they oppose to the elevated parts of the surface of the globe a resistance analogous to that of a well-defined north-east wind. For the same reason the trade-wind of the southern hemisphere blows from the south-east." (See Kämtz's 'Meteology,' p. 38.)

Sir J. Herschel, in his 'Meteorology,' thus explains this point. After speaking of the return of air from the north towards the equator, he says, "In this account of the production of wind, however, no account is taken of the earth's rotation on its axis, which modifies all the phenomena, and gives their peculiar character to all the great aërial currents which prevail over the globe." "To form a right estimate of its importance, it is only necessary to observe that, of all the winds which blow over the whole earth, one-half at least, more probably two-thirds, of the average momentum is nothing else than force given out by the globe in its rotation, in the trade-currents, and in the act of reabsorption or resumption by it from the anti-trades. Since the earth revolves on an axis passing through its poles from west to east, each point in its surface has a rotatory velocity eastward proportional to the radius of its circle of latitude, and any body of air relatively quiescent

on that point will have the same. Conceive such a body to be urged by any impulse in the direction of a meridian towards the equator. Since such impulse communicates to it no increase of velocity, it will find itself, at each point of its progress, continually more and more deficient in this element of movement, and will lag behind the swifter surface below it, or drag upon it with a relative westerly tendency. In other words, it will no longer be a direct north or south wind, but, relatively to the surface over which it is moving, will assume continually more and more the character of a north-easterly or south-easterly one, according as it approaches the equator from the north or south" (p. 57).

In both of these extracts it will be perceived that the great departure of the tropical trade-winds from a meridianal line is attributed to unequal rotatory velocities of the surface converting the north and south winds of the Atlantic to north-east and south-east winds; and in the Pacific changing their direction, until over the greater portion of its surface these become east winds, though this surface is within the tropics, where comparative rotatory velocity does not greatly alter. Yet all western tendency of air in those parts is attributed to the air being left behind by the rotation of the surface that is encountered. Now, that air may be left behind by unequal rotating surfaces is admitted. From the nature and action of physical force, it would seem that such must be the case. But is it so left to any definite or appreciable extent? The eminent writers quoted assert in general terms that the retardation does take place, without thinking it necessary to show in a scientific way what is its precise amount, or what will be the difference in the retardation in latitudes revolving at very different speeds. In short, it is assumed, in a general way, that retardation does take place, just as the ascent of air within the tropics was by Hadley,

in order to account for unexplained facts. But if no influence of the kind can be traced in parts near to the polar circles, say from 74° of latitude, are we at liberty at once to assume, without any attempt at proof, that that influence is great within the tropics? It appears an unwarranted assumption.

Even within the tropics, however, there is strong evidence to show the fallacy of the assumption that is here combated. Over the eastern side of the Atlantic Ocean, from Sierra Leone, in west longitude 15°, a wind generally blows with considerable force towards the east, in the Gulf of Guinea, to beyond the meridian of Greenwich; and therefore the air constituting the wind moves eastward faster than the surface over which it is passing; and there is good reason for presuming that it continues its eastern course far into the interior of the continent of Africa. The whole of this part is near to the equator, and the comparative increase of rotatory velocity is therefore not considerable; but the positive rotation here is very rapid, say, in round numbers, 1000 miles an hour. And yet, so far from the air being left behind by the surface, it moves so much faster as to make it a wind of considerable strength.

In like manner, on the eastern side of the Tropical Pacific Ocean, winds blow from the west towards the east, and of course the air in them must move eastward faster than the surface of the globe on which it presses. One of them blows with considerable constaney, another is intermitting in its action, but both are illustrative of the subject under consideration.

The former wind is generally found blowing from the west of California, in about latitude 30° north, to the equator in the Gulf of Panama; and therefore it passes eastward over about 40° of longitude faster than the surface of the earth does on which it presses. The other

wind alluded to, sometimes called a Pacific monsoon, is found in the southern summer blowing from the Society Islands to Guayaquil; and this is through (say) 86° of longitude; and, in the whole of this course, the air moves eastward faster than the surface of the globe.

It is only when the sun is in the southern hemisphere that this latter Pacific wind is found blowing from the west; but it then constitutes a regular monsoon, sometimes of considerable strength. At other times of the year, the ordinary eastern trade-wind prevails over the same range of the ocean. But if the east wind was caused by the rotation of the earth leaving the air behind it, what could cause the wind coming from the west to move towards the east faster than the surface? The rotatory velocity of the surface is the same at all seasons; and certainly it may be presumed that the cause, whatever it may be, which makes the air rotate more rapidly than the earth at one time, may be able to make it move either more or less rapidly at another time, and in a different direction. This, I have attempted to show, is the case in all winds,—the disturbing power which causes them being in action, in all cases, where the wind terminates.

Another wind is said to be found blowing near to the equator, across the whole Pacific Ocean from the west, into the Gulf of Panama, and of course moving eastward faster than the surface; but enough has been said to show that on the eastern sides of both the Atlantic and Pacific Oceans well-known facts are to be found, which are directly opposed to the theory that the rotatory motion of the globe palpably disturbs the movements of the air on its surface.

There are two assumptions generally made by meteorologists which it is here contended are the offspring of the imagination, and are in substance fallacious. One is, that winds are produced by the unequal heating of different

parts of the earth's surface; and the other, that the unequal rotatory velocity of different latitudes palpably modify and disturb the course of the winds. The former of these causes has been erroneously said to produce the monsoons of the Indian Ocean. This I have elsewhere shown to be an error, more particularly as regards the south-west monsoon. But the second assumption, which we are more specially considering, deserves further examination in reference to this part of the world. The north-west or winter monsoon of the Indian Ocean is first found over the ocean near to Arabia and Africa, moving towards the equator, and according to the laws of retardation, as laid down by modern meteorologists, it ought to become a north-east trade-wind blowing to the heated surfaces of Africa. It does not, however, do so; but from the mouth of the Red Sea and the Persian Gulf, near the northern tropic, it goes southward and *east*ward, passing near Hindostan, to the island of Ceylon; then turning more decidedly eastward, it passes forward to the equator, and terminates at the islands of Sumatra and Java, where heavy rains are falling. So that this wind passes through 23° of latitude over a sea cool compared with burning Africa, and in a direction the opposite to that in which Africa is situated, and at the same time traverses 40° of longitude with greater rapidity than the surface of the globe that sustains it! Instead of being left behind by the rotating surface, it travels eastward faster than that surface, moving steadily forward with increasing velocity to cloud-covered islands which are cooled and drenched by heavy rains.

But this is not the only part of the Indian Ocean where wind travels faster than the surface of the globe. When the sun has fully heated the southern hemisphere, a wind, called the petty monsoon, is found blowing eastward from the neighbourhood of Madagascar across the Indian Ocean

to the island of Java; and therefore, instead of being left behind by the globe, it, as in the cases of other winds, moves eastward at a quicker rate than the globe does. And here, as in the western monsoon of the Pacific Ocean, it is only at a certain season that this wind blows towards the east, although the surface of the globe revolves with equal velocity at all times. Thus, in the eastern portion of the Indian Ocean, from about the northern tropic to the equator, one wind is found blowing to the east, while another is found on the other side of the equator blowing in the same direction. So much are facts in this part of the world at variance with the assumptions which have been made by ingenious inquirers to sustain an erroneous theory.

There is another part which deserves notice, as bearing on the subject under consideration. The islands of the great Indian archipelago have the broad Pacific Ocean to the east on one side, and the open Indian Ocean on the other to the west; and near to and on each side of the equator, winds blow in opposite directions towards these islands. Thus, while winds from the east blow to them from the Pacific, other winds, as we have seen, blow from the west over the Indian Ocean. The winds change with the arrival and departure of the rainy season on the one or on the other side of the different parts of the great archipelago. And it is stated to be a well-known fact that wind blows through Torres Straits, within 10° of the equator, during one half of the year from the east and during the other half from the west. As statements to this effect are to be found in many books, it is rather surprising that meteorologists should continue to repeat the hypotheses that are so much at variance with them.

But it has been found in other departments of natural operation, as well as in this, that first appearances have given birth to erroneous views, which, in due time, have been worked into a fallacious hypothesis. This took place

in astronomy and also in chemistry; and the old theories long kept possession of the minds of men who, in their earlier days, were trained to adopt them. Meteorology seems to be at present in a state of transition; its defects, however, have recently been plainly and strongly characterized by more than one eminent philosopher, it having been said that it was an unintelligible mass, which became less comprehensible the more it was explained! And this may be truly said of the present popular theory to account for atmospheric disturbances.

I have, however, attempted to show that the tropical ocean or dry sun-heated land does not cause air to ascend from heated tropical regions to flow over to the polar, and when cooled return to the tropics on the surface, and also that air so passing is not to any palpable extent affected by the unequal rotation of different latitudes. It has also been proved, by a large amount of reasonable evidence, that all the great atmospheric disturbances are caused by the liberated heat of condensing vapour warming and expanding the gases, which are then forced upwards by heavier air, producing ascending currents with horizontal winds to supply them. In this way it has been shown that the trade-winds and monsoons of the tropics and all the more violent winds and storms over certain parts of the ocean, as well as the winds of other parts, are produced. Evaporation of the small particles of water, which constitute floating cloud, makes the air heavy where that evaporation takes place, when the local air sinks through a lighter part of the atmosphere to the surface, over which it spreads, creating moderate movements of the air, such as cool breezes. The former disturbances, constituting the great winds, are effects of limited portions of the atmosphere being heated, expanded, and rendered light, when they are forced into the higher regions. The latter and smaller winds are results of portions of air being cooled and made

heavy by cloud-evaporation, when they sink to, and spread over, the surface. These two operations of local heating and cooling of the gases, it is contended, produce those movements which are called breezes, winds, and storms. And if we started from the process of evaporation of water from the ocean sending vapour into the atmosphere, and then traced its ascent and diffusion through the gases, and its condensation by their low temperature in the upper regions liberating the heat of the vapour, all the great disturbances that take place might be followed and rendered comprehensible and clear. And if, afterwards, the water which was produced by condensation of the vapour, but which did not fall as rain, were followed through its evaporation in the upper regions, cooling the local gases, and making them sink and spread out over the surface, all the disturbing processes might be exhibited in a natural and simple order, and meteorology, instead of being denounced as incomprehensible, might take its place with geology, zoology, and other sciences that are regularly taught in our schools.

XVIII.—*On the New Red Sandstone and Permian Formations, as Sources of Water-supply for Towns.* By Edward Hull, B.A., F.G.S., of the Geological Survey of Great Britain.

Read December 30th, 1862.

It was remarked by the late Dr. Buckland, that most of the large manufacturing towns of the central and northern counties are built upon the New Red Sandstone. This circumstance, which to the casual observer might appear accidental, seems, on closer inspection, to be attributable to the natural advantages which such a situation affords. For a manufacturing town, coal is necessary; therefore these towns

lie as close as may be to the coal-fields without actually standing on them—an advantage only to be fully appreciated by those doomed to pass their lives between collieries on one side and factories on the other. Besides this advantage, there is that of dryness. The New Red Sandstone is extremely porous; rain rapidly sinks into it, leaving a dry soil. The formation also yields building-stone, suitable for all kinds of rough or ornamental work. The sandstones of the Bunter are largely employed for the former; those of the Keuper for the latter. Nearly all the best freestones used in ecclesiastical or secular structures—from the Anglo-Norman period downwards—in the central counties, have been hewn from quarries in the Lower Keuper Sandstone. The last advantage to which I shall allude is one which is often unwittingly enjoyed or neglected by many towns—that of water-supply. Under and around all the towns built on this formation (or on the Permian) there lie natural reservoirs of pure water, which are often overlooked in the search for this most necessary element of manufacturing industry and ordinary existence. And thus most of the sites occupied by such towns as Manchester, Liverpool, Stockport, Macclesfield, Leek, Nottingham, Derby, Wolverhampton, Birmingham, and Kidderminster unite in themselves the advantages of easy access to coal, water, and building-stone.

To those who are acquainted with the difficulties and expense to which many of these towns, and others similarly situated, have been put when in search of water, it may appear strange that they have often overlooked, or failed in taking full advantage of, the supply of pure water which Nature, that thrifty housewife, has pent up in the rocks. Such, however, is the case; and just as the question of the supply of coal *below* the New Red Sandstone is every day becoming more important, so is also the question of the supply of water *within* the same formation receiving increased attention.

The formations to which these observations refer are largely distributed over the Midland, Western, and North-Midland counties. In geological position they are included between the Coal formation below, and the Keuper, which forms the upper division of the New Red Sandstone, and which yields the brine-springs and rock-salt of Cheshire and Worcestershire. From this formation the brine at Rugby and Cheltenham has undoubtedly been derived, by ascent through the overlying Lias. Under these towns the true fresh-water-bearing strata are in all probability entirely absent, and will not be found south-east of a line drawn from the Bristol Channel to the mouth of the Humber. This is owing to the attenuation or thinning out of the Triassic formations towards the south-east of England—a most interesting fact in physical geology, and one to which I drew attention in a paper published by the Geological Society of London*. Its bearing on the question of water-supply should never be lost sight of.

These changes in the thickness and distribution of the Trias and Permian beds are represented in Fig. 1. It will be observed that, out of five subformations resting directly on each other, all except that marked 2 are water-bearing; and also how these strata attain their greatest vertical development in Lancashire and Cheshire, and thin away in the direction of the mouth of the Thames.

The general succession of the series is as follows:—

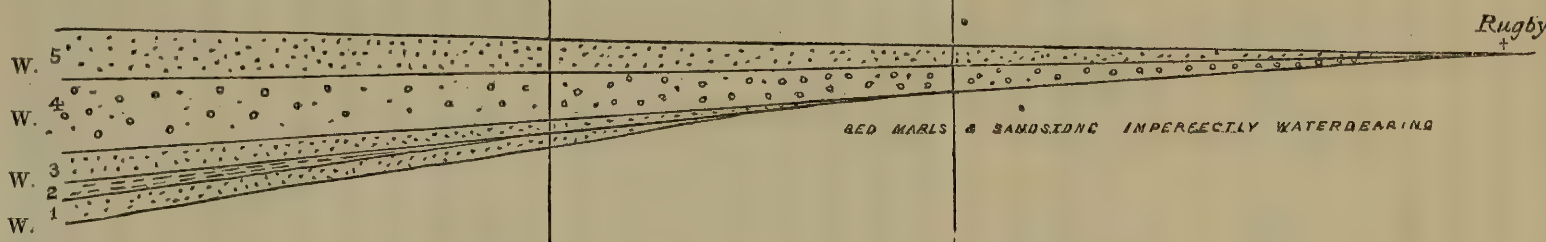
New Red Sandstone	5. *Upper Mottled Sandstone.*—Fine soft incoherent sandstone.
	4. *Pebble-beds.*—Reddish-brown pebbly sandstone, used for building, becoming a loose conglomerate in Staffordshire.
	3. *Lower Mottled Sandstone.*—Fine soft incoherent sandstone.
Permian Beds	2. *Red Marls*, with limestone (not water-bearing).
	1. *Lower Red Sandstone.*—Soft fine red sandstone.

The Lower Permian sandstone, a nearly homogeneous rock of extreme porosity, attains considerable thickness in

* "On the South-easterly Attenuation of the Lower Secondary Rocks of England," Quart. Journ. Geol. Soc. vol. xvi.

Fig. 1.—*Diagrammatic Section from the North-west towards the South-east of England (showing the manner in which the water-bearing beds of the Trias and Permian thin away in the direction of Rugby and the Mouth of the Thames).*

N.W. Lancashire and Cheshire. Derbyshire and Staffordshire. Leicestershire and Warwickshire. S.E.



	Lancashire and Cheshire.	Derbyshire and Staffordshire.	Leicestershire and Warwickshire.
N. R. Sands.	5. Upper Mottled Sandstone, 300 to 400 ft.	50 to 200 ft.	0 to 100 ft.
	4. Pebble-beds, 500 to 750 ft.	Conglomerate, 100 to 300 ft.	0 to 150 ft.
	3. Lower Mottled Sandstone, 0 to 200 ft.	0 to 100 ft.	Absent.
Permian.	2. Red Marls with Limestones	2. Absent	Absent.
	1. Lower Red Sandstone, 50 to 450 ft.	1. Probably represented by Red Sandstones and Marls (sometimes water-bearing)	Red Sandstones and Marls of great thickness.

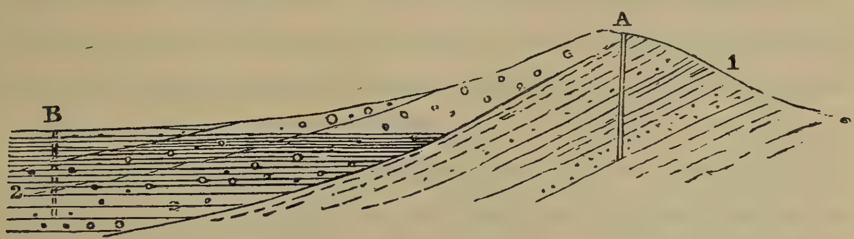
Note.—The formations marked W are water-bearing.

the north of England and Lancashire, as shown by Messrs. Binney and Harkness. Wherever it occurs, it will probably be found well charged with water. It also attains a development of 200 feet in Durham, as shown by Professor Sedgwick, forming the base of the escarpment which marks the boundary of the magnesian limestone; but in the central counties it disappears, and in its stead we find a thick series of red calcareous marls and sandstones, occupying large tracts of Warwickshire, Staffordshire, and Worcestershire, resting on the Coal-measures, and overlain by the New Red Sandstone. The relative position of these beds and the Lower Permian sandstone cannot be satisfactorily determined, as they nowhere occur in the same place; but it is probable that the latter is more recent than the former, though they are both of Permian age. As a group of water-bearing strata, however, the red marls and sandstones of the central counties must be considered as ill adapted for this purpose, as the frequent occurrence of thick beds of impervious marls and clays must prevent any large accumulation of water in the beds of sandstone; and whatever the quantity of water, it is likely to be hard, owing to the large distribution of calcareous matter throughout the whole of the formation. It was into these beds that the well for the supply of Wolverhampton was sunk, and which failed to obtain a sufficient supply (see fig. 2).

The lower division of the Keuper, called Lower Keuper Sandstone, which lies at the base of the great series of red marls, I have omitted to include in the category of water-bearing strata, as they are only imperfectly so, and might often be found to yield salt water. This series of beds consist of white and brown sandstones, tolerably hard, interlaced with red shales and clays, the former being permeable, the latter not so. On this account these strata frequently give rise to springs, and are in consequence called "water-stones" in Cheshire and Lancashire. The

frequent partings of shales and clays, taken in connexion with the comparatively small vertical development of these beds, must prevent them from occupying as high a position in the scale of water-bearing strata as the underlying Bunter Sandstone.

Fig. 2.—*Diagram to show the position of the Well at Wolverhampton which failed to afford a supply.*



1. Red Marls and Sandstones of Permian age—only moderately water-bearing.

2. New Red Sandstone and conglomerate—highly water-bearing.

A. Actual position of well which failed. *B.* Position where it ought to have been. If the well had been sunk at *B*, I have no doubt a large supply could have been obtained, which might then have been pumped up to the reservoir on the top of the hill at *A*.

The following is the general succession of the Triassic and Permian formations in the Midland and North-western counties to which reference is made in these pages, showing which are water-bearing, or the contrary.

General Succession of the Triassic and Permian Beds.

		Lancashire and Cheshire.	Derbyshire, Staffordshire, and Notts.	Warwickshire and Leicestershire.	Remarks.
Trias or New Red Sandstone.	Keuper.	5. Red marl, 3000 ft.	600 ft.	450 ft.	Brine.
		4. Lower Keuper sandstone, or waterstones, 450 ft.	200 ft.	150 ft.	Moderately water-bearing.
	Bunter.	3. Upper mottled sandstone, 400 ft.	50 to 200 ft.	Absent	Water-bearing.
		2. Pebble-beds, 500 to 750 ft.	100 to 300 ft.	0 to 100 ft.	Water-bearing.
		1. Lower mottled sandstone, 0 to 200 ft.	0 to 100 ft.	Absent	Water-bearing.
Permian Beds.	Upp.	2. Red marls and limestones	Absent	Absent	No water.
	Lower.	1*b*. Lower Red Sandstone, 50 to 450 ft.	Absent	Absent	Water-bearing.
		1*a*. Absent	Red sandstones and marls, 500 ft.	1500 ft.	Moderately water-bearing.

It will be observed from the above that out of eight sub-formations four are water-bearing, two are moderately so, and one produces brine, at least generally.

The excellence of the Triassic and Permian beds above named, as regards their *water-bearing* powers, depends principally upon the three following qualities. 1. Porosity. 2. Homogeneity or uniformity of mineral composition. 3. Filtering-power; to which may be added the occurrence in only very small proportions of lime or other soluble minerals. We shall now briefly discuss each of these qualities.

1. *Porosity.*—This is a quality inherent in all sandstones. In proportion to their greater or less solidity, they allow the passage of water throughout their mass with less or more freedom, and they absorb water with which they are brought in contact to the point of saturation. The sandstones of the Trias are on the whole of an open and incoherent character; and in some districts, where the "pebble-beds" of the Bunter pass into an unconsolidated conglomerate, as on Cannock Chase, all the rain that falls is immediately absorbed, except what is given back by evaporation. Thus the New Red Sandstone is ever receiving, and drinking in, supplies from the clouds, which percolate downwards and accumulate in the lower strata till a water-level is formed, which oscillates according to the rainfall and the extent and number of the springs, which are the natural sluices. When these porous strata rise to the surface, and then plunge beneath the impervious formation of the Red Marl, there can be no doubt they are saturated with water to considerable depths, and to great distances from their outcrop. In this position the water might be reached by artesian wells, were it not for the danger of tapping brine-springs in passing through the formation of the Red Marl. This capacity for forming large reservoirs of water within the mass of the formation is one of its most important

qualities, and depends also to a large extent on its homogeneity, which we now proceed to consider.

2. *Homogeneity.*—The three members of the Bunter division (see above) are very nearly similar in composition, though somewhat differing in solidity.

The first and third members consist of soft incoherent sandstone, very porous, and readily absorbing whatever rain falls on its surface. The middle member is generally more compact, but in the central counties often passes into a loose conglomerate of quartz-pebbles, also of an extremely porous nature. Therefore, for the purposes of water-supply, the whole formation, attaining a vertical thickness of several hundred feet, may be considered as equally absorbent throughout, being in effect (with occasional exceptions) a natural reservoir, the water from which is capable of being utilized by mechanical skill. At the same time it must be recollected that judgment and knowledge to determine the most effective mode of applying that skill are as necessary in this case as in laying out a colliery, or any similar undertaking.

That this formation readily yields to the passage of water, both vertically and horizontally, is proved by general experience. The case of the well sunk at Green Lane, near Liverpool, and belonging to the Corporation, illustrates this point. This well, by sinking and boring, reaches a depth of 385 feet; and I am assured by Mr. Duncan, the engineer, that the pumping operations influence wells "several miles distant." In fact, I believe it has laid dry all the wells within a radius of a mile. Mr. J. Cunningham, of Liverpool, who has paid much attention to this subject, informs me that some wells in that town, sunk for a quarter to half a mile from the sea-side, have now become so salt as to be useless for household purposes, proving that the sea can make its way inland to this distance at least. Upon this point the evidence is perfectly conclusive; for we find

that on both sides of the Mersey the water contained in the New Red Sandstone acts as a counterpoise to that of the sea, so that when, by pumping, the water-level of the rock is lowered, and the pressure removed, the sea forces a passage for itself through the strata in order to restore the balance.

But the most conclusive proof of the extreme porousness of the formation is derived from the large quantity of water which it is capable of yielding from a single well, and which therefore must be drawn from a considerable area. As instances we may mention the Windsor Well at Liverpool, which yields 1,103,000 gallons per day; the Green Lane Well, which gives no less than 3,321,000 gallons; and the Gorton Well, near Manchester, 864,000 gallons. It has also been found that the supply increases with the depth, of course in various proportions, as a larger area is thereby drained. This is a most valuable property of the formation. In other geological formations composed of constantly varying materials, the effect of increasing the depth might be to pass from a water-bearing stratum, such as sandstone or limestone, to one which contains no water at all, such as shale; but it is otherwise with the New Red Sandstone; and thus, when a small supply only is required for a small town or a factory, that supply may be increased by deepening the well when the increase of demand so requires.

For the purposes here referred to, there are no formations in England better adapted than the New Red Sandstone; for if we compare with it the Chalk and Lower Greensand, the great water-bearing strata of the South of England, it must be allowed to excel the former in the greater softness of the water, and the latter in having a tenfold greater horizontal and vertical development.

The freedom with which water can percolate both late-

rally and vertically through this formation is due to uniformity of composition. In some parts of Cheshire and Lancashire we may estimate the thickness of the Bunter sandstone at 2000 feet; in the central counties, at 500 to 1000 feet; and throughout this mass of porous sandstone there seldom occurs a stratum which is impervious to water. We find, it is true, here and there, bands of shaly marl, but they are very thin, and never extend over a large tract of ground. Hence it is that wells, properly situated with respect to the dip of the beds and the area of rainfall, can drain such large tracts, and when of sufficient depth are not likely to be affected by dry seasons.

3. *Filtering power.*—Pure sand and gravel are the most general agents employed in filtering on a large scale; and by means of the New Red Sandstone and similar deposits, Nature accomplishes the important function of extracting from water, derived from various sources, all noxious impurities, and returning it back again, through her springs and fountains, clear and fitted for our use. Such is the character of the water derived from this formation. It is generally very free from matter in mechanical suspension, nor is it often impregnated strongly with salts in solution. Carbonate of lime is often present, but in small quantities. Iron, which sometimes occurs in the water raised from wells, can readily be precipitated by allowing the water free access to the open air. Brine is frequently diffused through the Red Marl, which forms the upper division of the Trias; yet I have only known of one instance out of a large number of wells where it has been found in the Red Sandstone. The case referred to occurred at Ordsal, near Manchester, and is mentioned by Mr. Binney, F.R.S., in a paper recently read before this Society. The purity of the water, and its fitness for manufacturing as well as domestic purposes, is most satisfactorily shown, from the fact that it is used in bleaching, dyeing, and other works in

Manchester and Stockport, and for household use in Liverpool, Birkenhead, Southport, St. Helens, Birmingham, and many other large towns, though not to the extent of which it is capable. I may also mention, on the authority of Mr. Ramsbottom, that one of the best sources of supply for locomotive engines on the London and North Western Railway is drawn from a well in this formation at Warrington Junction in Lancashire. Now, for engine boilers pure and soft water is absolutely necessary. I may also appeal to all persons who can enjoy a glass of cold water for their verdict, if there is not as much difference between a draught from a deep well and one from a moorland reservoir, as between a glass of Burton ale newly drawn and one which has stood till it has become "flat, stale, and unprofitable"*.

With all these advantages afforded by the New Red Sandstone as a source of water-supply, it is remarkable that many large towns, with these facilities at hand, have preferred going to long distances for water collected from surface-drainage, which must always be inferior in quality and variable in quantity, in comparison to that drawn from the internal reservoir of the rocks. The cases of Liverpool and Stockport may be particularly mentioned. One special advantage in reference to water drawn from wells over that derived from streams is, that it is less subject to variation in quantity, as it is less influenced by the

* The opinions regarding the quality of the water derived from the Trias and Lower Permian sandstone vary much. In some cases the water has been found to be hard, though derived from a series of strata containing very small quantities of carbonate of lime. Dr. Angus Smith states, as the result of his examination of the waters from the Manchester wells, that they contain 14 grains of lime per gallon, namely, 8 grains of sulphate and 6 of carbonate. It is to be recollected, however, that most of these wells enter the New Red Sandstone, pass down through the Permian marls, which are highly calcareous, and draw their supply from the Lower Permian sandstone. A portion of the lime met with may therefore be derived from the intermediate marls, which contain beds of limestone and gypsum.

vicissitudes of long droughts. When these occur of more than ordinary length, the brooks dry up and the water fails; but in the underground reservoirs of the strata, the circulation of the water is so slow throughout the mass that the supply is economized, and is capable of holding out for lengthened periods. Yet both the towns above-named have preferred placing reliance on these variable sources in preference to those of a more constant character.

The advantages afforded by the New Red Sandstone have often been thrown away by an improper selection of positions in sinking wells. Through ignorance of the geological structure of the district from which it is proposed to draw a supply, large sums of money have been uselessly expended. The case of Wolverhampton may be instanced. This town, naturally well placed for drawing a supply of water from the New Red Sandstone, has been obliged to have recourse to a river many miles off to minister to its necessities—a result due to the selection, in the first instance, of an improper site for the well (see fig. 2). The case of Rugby was one of another kind. For here there was *no* New Red Sandstone from which to draw, and the discovery of salt water in the strata of the Red Marl might have been anticipated, as it is the same formation which produces the brine-springs of Cheshire and Worcestershire. (See diagram, fig. 1, p. 259).

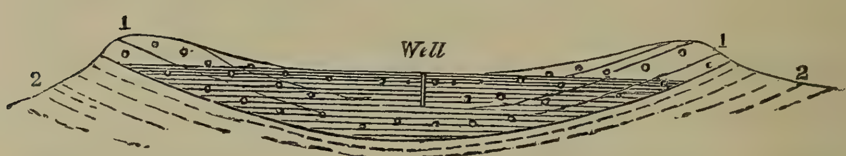
How to find water in the natural reservoirs of the strata is every day becoming more and more a geological question. It cannot be too strongly urged that, in order to solve this problem, a knowledge of the composition, properties, and structure of the rocks is indispensable—just as much so, in fact, as in the question of finding coal. The following suggestions may therefore be of service as rules of general application:—

It may be almost always expected that the water will

tend to flow in the direction of the dip of the strata; therefore it is necessary that the well should be placed as far as possible from the outcrop or margin of the formation.

The most favourable position for a well is in the centre of a basin or trough, for towards this point the water will tend to flow; and if the boundaries of the formation round the margin of the basin happen to be upon a higher level than the centre of the basin, the water will in all probability rise to the surface by its own pressure. Such was the result in an experiment made by Mr. Bateman during the formation of the new Wolverhampton Waterworks, near Tonge, and also in one which was made under the author's direction at Whitmore Station, for the supply of the works of the London and North-Western Railway at Crewe (fig. 3).

Fig. 3.—*Section to show the position of the Well at Whitmore, Stafford.*



1. New Red Sandstone. Water-level shown by the shaded part. The water rose four feet above the railway cutting.

2. Permian Marls. Red Marls,—impervious to water, and preventing it sinking downwards.

I may here be allowed to give a brief account of this experiment, as it illustrates the point in question.

It was the wish of the company to obtain water within the present bounds of their property, the present supply being insufficient, and the space occupied by the reservoir being required for other purposes. After a short survey, I fixed on a point 200 yards south of Whitmore Station as likely to yield as large a quantity of water as could possibly be required, both for the works and the town. This

point is near the centre of a trough or half-basin of New Red Sandstone. The hills of this formation rise in a semicircle around it, and towards this point the strata dip. The rock is generally very incoherent, and nearly all the rain which falls within a radius of a mile and upwards must, I concluded, flow in the direction of the point selected. These expectations were fully verified. A 4-inch bore having been sunk only 148 feet, a column of water ascended with force to a height of 4 feet. It was at first strongly impregnated with iron, but gradually became limpid and clear, and has flowed ever since. A well is now being sunk, and the water will be carried by gravitation to Crewe in pipes.

Faults or dislocations in this formation do not act as barriers for preventing the underground flow of the water, as in the case of coal-mines. They simply retard its flow through the strata, or form ducts along which to guide it. For this reason the lines of dislocation offer favourable sites for wells.

The New Red Sandstone is frequently underlain by impervious clays or marls belonging to the coal-measures or Permian beds. In such cases the water must accumulate towards the bottom of the sandstone, as its further descent is arrested. When this arrangement of the beds takes place, the prospect of a large supply may be considered favourable. The supply will also be influenced by other causes, such as the continuous area of the formation, and the presence or absence of drift-deposits overlying the sandstone. A thick coating of boulder-clay probably prevents the rain ever reaching the rock beneath it.

In conclusion, I think we may assert with confidence that the value of the New Red Sandstone as a source of water-supply is as yet imperfectly appreciated, and that many of the towns still suffering from the want of that most necessary element, good water, would do well to

have recourse to the supply which Nature has placed within their reach.

I now proceed to give some special instances of wells in the Trias and Permian formations.

Liverpool District.

Well at Bootle.—Surface 50 feet above sea. Four holes. Greatest depth of one of these reached, in 1844, 600 feet. Yielded at first a large volume of water; but the quantity was never accurately determined till January 1850, when the yield was 1,102,000 gallons per day. Now reduced to 700,000 gallons per day.

Windsor Well.—Sunk about the year 1840. Surface 190 feet above sea. Depth of well 210 feet. Yield not ascertained till 1850, and was then found to be 700,000 gallons per day. Bored a hole 4 in. diameter, 189 feet from the bottom. Yield then increased to 958,000 gallons per day. In June 1853 the yield diminished to 814,000 gallons per day. Then widened the 4-inch bore to 6 inches in diameter, and deepened to 210 feet. Yield increased to 1,110,000 gallons. In 1856 diminished to 972,000 gallons. Bored additional depth of 34½ feet. The total depth is now 245 feet, and the yield about 1,103,000 gallons per day.

Green Lane Well.—Sunk 1845–6. Surface 144 feet above sea. Depth of well 185 feet. Yield at first 1,250,000 gallons per day. In April 1852, yield 1,203,000 gallons. Bored 6-inch hole, 60 feet from the bottom of well; yield increased to 2,317,000 gallons.

June 1853, yield 2,303,000 gallons. Bored again 38½ feet; yield increased to 2,689,000 gallons. June 1856, widened hole, and deepened it to a depth from bottom of well of 200 feet. Yield increased to 3,321,000 gallons per day. Total depth from surface 358 feet.

Messrs. Earl and Carter's well, in Oil-street, above a

quarter of a mile from the river, became useless through the filtration of salt water. This has also occurred in several other cases here and at Birkenhead.

Several successful wells and borings have been sunk under the direction of Mr. Bateman and Mr. Cunningham for the supply of Birkenhead and the neighbourhood. One of them, at Flaybrick, reaches a total depth of 360 feet, and yields upwards of 2,000,000 gallons per day.

St. Helens is supplied from a well sunk on Eccleston Hill, 70 yards in depth in New Red Sandstone, at a height of 260 feet above the sea.

The towns of Southport and Ormskirk are also supplied from the same formation.

At *Preston Junction*, on the London and North-Western Railway, an abundant supply of very pure water is obtained from a well about 40 feet in depth. The water answers well for engine boilers.

Manchester District.

Gorton Waterworks.—Two wells, at about 50 feet from each other, communicating by a tunnel. Depth of the pumping-well, 210 feet; 12 feet in diameter. From the bottoms of the wells tunnels are driven out, all in New Red Sandstone. Yield, 864,000 gallons per day. Not in constant use.

There are in Manchester and Salford from 60 to 70 deep wells driven through the New Red Sandstone and Permian formations, and yielding probably 6,000,000 gallons per day*. The water is employed in bleaching, dyeing-works, and breweries; and though harder than that which is supplied from the Yorkshire moors, is well adapted for

* The strata pierced by some of these wells and borings are described by Mr. E. W. Binney, F.R.S. See Trans. Geol. Soc. vol. i.; and the same author, "On the Permian Beds of N.W. England," Mem. Lit. & Phil. Soc. Manchester, vol. xii.

some of these purposes. It has often appeared to me extremely difficult to account for so large a supply from an area not greater than seven square miles, and for the most part debarred from access of rain-water by buildings, and a thick coating of boulder-clay. I am disposed, however, to think that some of the water finds its way from the rivers Irk, Irwell, and Medlock, and in its passage through the sandstone is freed from the chemical and mechanical impurities which have changed those rivers into filthy sewers. The following are the cases in which I have been able to measure the supply, through the kindness of the proprietors.

1. *Messrs. Worrall's Dye-works,* Old Garratt.—Depth, 109 yards in New Red Sandstone. Yields 384,480 gallons per day.

2. *Messrs. Hoyle's Works,* Mayfield.—Passes through the following beds, as described by Mr. Binney:—

	ft.	in.
New Red Sandstone	143	4
Permian marls, with bands of limestone, &c.	153	9
Lower Permian sandstone	59	4

The yield is considerable; but the pumps being out of use at the time of my visit, it could not be determined.

3. *Mr. J. Clemson's Dye-works,* Horrocks.—New Red Sandstone, 10 yards; Permian marls, 80 yards. Lower Red Sandstone, soft, with much water. Two 4-inch bore-holes, and chambers in the rock at 23 feet from surface. Yield, 262,080 gallons per day.

4. *Mr. Boddington's Brewery,* Strangeways.—Yield, 55,840 gallons per day.

5. *Mr. Charlton's Works,* Salford.—About 150 yards from last (No. 4). Shaft 70 feet; at the bottom several large chambers and bore-holes. Yields 348,000 gallons, in 16 hours, per day.

6. *Mr. Smith's* (late *Mr. Joule's*) Brewery, Salford.

		yards.
New Red Sandstone (about) . . .		156
Permian	marls with limestone . .	40
	rock and clay alternating .	10
	hard sandstone (with water)	

There are two pumps, which can be kept at work for 48 hours, yielding at the rate of 137,000 gallons per day. Only one, yielding half that quantity, is at work. Large chambers are excavated in the New Red Sandstone. The water-level is nearly that of the River Irwell.

7. *Messrs. Bury's Dye-works,* Salford.—Depth of well and bore-hole about 100 yards. Two wells, only a few yards' distance from each other. One yields 353,240 gallons per day, and the other 66,240 gallons.

8. *Messrs. Mosely's Dye-works,* Salford.—A large engine, which pumps about 1,500,000 gallons per day. This well is only about a quarter of a mile from the last. The proprietor declined to allow the author to make the necessary admeasurements for an exact computation.

9. *Messrs. Worrall's Works,* Ordsall.—This well and bore-hole, 460 feet deep, produced salt or brackish water at the bottom, and gave employment to four pumps. After an examination of the spot, I feel convinced that the cause of the saltness arose from the fact that this well is sunk further on the dip of the New Red Sandstone than any other in the neighbourhood. In consequence of this, the "dead" or stagnant water has remained pent up in the rock for ages, and has thus become impregnated with all the salts which the rock contains. There is every reason to believe that by a continuation of the pumping the salts would be gradually dissolved and carried away. The section was entirely in New Red Sandstone. The well was pumped for 12 months, yielding at the rate of 717,120 gallons per day.

10. *Broughton Road Paper-works.*—This well has been recently sunk, and gives the following section:—

	feet.
1. *Stiff, close, and hard stuff* (probably Drift and New Red Sandstone)	240
2. *Red loam with mixtures of clay and shale* (probably Permian marls)	210
3. *Soft Red Sandstone* (Permian sandstone) . .	150
4. *Hard bands* (probably Coal-measures) . . .	120
	720

This well yields about 100 gallons per minute, the water rising to the surface *.

11. *Collyhurst Sand Delf.*—Well in Lower Permian sandstone. Water hard, but transparent, exhausted after 12 hours' pumping, and yields 260,064 gallons per day.

12. *Artesian Boring.*—A bore-hole at the works of Mr. John Wood, at Medlock Vale, passed through the following strata:—

	ft.	in.
Alluvial gravel	26	0
New Red Sandstone	23	0
Permian marls, with bands of gypsum and limestone	246	3
Lower Permian sandstone	375	11
Coal-measures	90	0
	761	2

On reaching the Lower Permian sandstone, the water rose to the surface and flowed over with a strong head.

Stockport.—Numerous borings for water have been made in this town, as stated by Mr. E. W. Binney (Trans. Geol. Soc. Manchester, vol. i.), to a depth of 100 yards, at which level the supply has been found abundant. The

* For the particulars above stated I am indebted to Mr. John Knowles.

pebble-beds of the New Red Sandstone here rest immediately upon the Lower Permian sandstone, and the thickness of the water-bearing strata is therefore very great, probably not less than 1000 feet in many parts. Few towns seem better situated for drawing their water-supply from these rocks, which must be highly charged. A copious spring bursts forth in the bed of the Mersey, about a mile above the town, out of the Lower Permian sandstone.

The town of Cheadle in Staffordshire is supplied from a well sunk in an outlier of New Red conglomerate of about a square mile in extent, which rises to a height of about 150 feet at the back of the town. Doubts were expressed at the commencement of the undertaking whether a sufficient supply could be obtained from an area of so small an extent, which is also partly drained by springs, bursting forth round the base of the hill, at the junction of the sandstone and coal-measures. The supply, however, has proved sufficient.

The last instance I shall adduce has reference to springs in the neighbourhood of Leek. This town is built upon the north end of a long tongue of New Red Conglomerate, which lies in an old trough of Carboniferous Rocks. The town itself is supplied from surface-water collected on the moors to the north of the town, but to the south of the town a splendid spring bursts forth from a knoll of New Red Sandstone, from which all the pottery towns, except Longton, Fenton, and Stoke, are supplied. A description of this spring is given by Mr. T. Wardle*, F.G.S. The water is pumped by engines from the valley up to Ladderedge Reservoir, a height of 287 feet, and is distributed by pipes to the various towns. The engines are capable of pumping 3,000,000 gallons into the reservoir daily; and Mr. Elliot, the engineer, considers that the

* Geology of Leek, p. 263.

springs are capable of yielding this amount. The supply is perennial, and the springs are probably fed by infiltration from the Churnet. There are several other springs bursting forth from this small area of New Red Conglomerate.

XIX.—*On Ocean Swell.*

By Thomas Heelis, Esq., F.R.A.S.

Read January 27th, 1863.

Besides the undulatory movements which are impressed upon the surface of the ocean by the winds prevailing at the time and place of observation, which movements are known by the name of waves, there are other undulations met with at sea which merit particular attention and study, and which are distinguished by the name of swell.

These swells are either regular or confused, according to the causes from which they arise. In some cases the same cause produces a confused and also a regular swell.

The causes of swell are :—

The direction impressed upon the undulations of the water by winds blowing for a long time in the same direction.

The existence of a current. This is almost always found to produce a swell running towards a point of the compass opposite to that towards which the current is flowing. In log-books the current is always noted as flowing *towards* a point, the swell as running *from* a point, so that current and swell appear to be of the same name.

These two descriptions of swell are in their nature regular, but that arising from current may be disturbed and become confused when it sets in a direction different from that of the winds prevailing at the time and place of ob-

servation. In such a case there are, in fact, two swells—one arising from the wind, and one from the current.

The remaining causes of swell are:—

The undulations thrown off from a cyclone in its progress. These occasion a very confused sea in the area occupied by the cyclone itself, and a confused swell at a short distance beyond its limits; but this description of swell has the peculiarity of becoming more regular as its distance from the cyclone which causes it is increased. The reason of this will be obvious from a consideration of the diagrams given in Lieut.-Colonel Reid's account of the Progress of the Development of the Law of Storms and of the Variable Winds (Ed. 1849), pp. 36, 37.

The swell thrown off by cyclones often travels to great distances. Several instances of this are given in Reid's work, and can be supplied by the experience of every seaman who has had any experience of tropical navigation.

The undulations impressed upon the waters of the ocean by an earthquake form the last class of swells whose causes are known to us. One of the most remarkable instances of undulations arising from this cause is afforded by the swell caused by the earthquake in Japan, which destroyed the Russian frigate 'Diana.' These undulations travelled across the whole breadth of the Pacific Ocean, and were detected by the tide-gauges of the United States Coast Survey at San Francisco.

All well-kept log-books of ships record the occurrence of swell, noting also its direction. In my own log I have generally added the observed height of it when considerable.

A heavy swell in open water is much more impressive, and gives to the thoughtful observer a much greater idea of power than even the waves in a heavy gale of wind. Instead of the ridges being formed by detached summits with intervening depressions, as in the case of a wave-ridge, they extend in one unbroken wall of water as far as the eye can reach.

There are other swells noticed in different parts of the world, which, so far as our present knowledge extends, cannot be traced to any of the above causes. Such swells occur particularly in the Southern Atlantic, and form the rollers at Ascension, St. Helena, Tristan d'Acunha, and the Island of Ichaboe, on the coast of Africa. They roll heavily into the Bay of Jamestown at St. Helena, which is open to the westward, often doing much damage to the shipping lying there. They are pretty regular in their occurrence, but are heavier on some occasions than others. The anchorage at Ascension, which is subject to them, is also open to the N.W. But little mention is made of them in Horsburgh. I have only heard of one instance in which these rollers have been met with at sea, and I am now unable to refer to it. The ship which encountered them on that occasion had her decks swept. Their cause is unknown; attempts have been made to explain them, by the supposition that they are caused by submarine earthquakes, which are known to be common a little to the south of the equator; but their occurrence is too regular to admit of such an explanation.

The altitude of the successive undulations in the case of swell observed in open water is variable; the largest observed usually occur in pairs or triplets—every tenth or twelfth undulation, or thereabouts, being large, while the intermediate ones are smaller, with a tendency in the sixth or seventh to be above the average of the small ones in altitude. Some observations which I have made in light winds, give altitudes for the largest undulations of 14 feet measured from the troughs, with a width of trough of some 600 feet, and a speed of translation in one case of twenty-two miles per hour.

An exact knowledge of the phenomena presented by swell (of which at present we know little or nothing) is a necessary foundation for any enlarged study of waves. The ocean from the Cape of Good Hope to 37° south, a

district in which the L'Agulhas current and its branches flow against the prevailing winds, should present phenomena differing from those experienced south of that parallel, where the current and prevailing winds flow in the same direction; and again, the phenomena presented in the district passed over in the inshore or homeward passage round Cape Horn would differ from those presented further to the southward.

The following observations have been made in ships on board of which I have made passages, and whose log-books I have had opportunities of consulting:—

The observations made on board the 'Jason,' 'Thunder,' and 'City of Pekin,' were recorded by myself. I have added in the column headed Remarks, the causes of the swell experienced in all cases in which such causes were ascertained at the ship.

Date.	Ship's position.	Nature and direction of swell.	Remarks.
1858. April 8.	18° 36′ N. 26° 3′ W.	Heavy swell from North, with strong ripplings on water.	Current N. 28°, W. 14′.
April 28.	26 47 S. 25 33 W.	A very confused swell from East.	
May 15.	39 33 S. 32 45 E.	A heavy swell from S.W.	From 6 p.m. of 10th May, gale and strong breeze from S.W. and W.S.W.
May 20.	38 36 S. 53 57 E.	A heavy swell from S.W.	
June 1.	26 18 S. 84 50 E.	A very heavy swell from N.N.W.	
1859. April 10.	26 27 S. 27 0 W.	A heavy sea from southward.	Wind all round the compass, but principally from S. E.; squally. Several previous days calm.
April 16.	33 18 S. 23 18 W.	A heavy sea from S.S.E.	Fresh breezes from E., and rainy. Current S. by W. 10′.

Screw Steamer 'Jason.'

Date.	Ship's position.	Nature and direction of swell.	Remarks.
1861. Nov. 22.	40° 12′ N. 13° 19′ W.	Southerly swell	Wind during this and two previous days from E.S.E. to N.E.
" 23.	38 34 N. 13 52 W.	Westerly swell	Wind from W.
" 24.	35 53 N. 14 35 W.	Westerly swell	Calm weather.
" 25.	33 48 N. 15 38 W.	Westerly swell	Calm, and N.E. wind.
Dec. 17.	26 5 S. 25 41 W.	Easterly swell...............	Wind easterly.
" 23.	34 41 S. 8 15 W.	Heavy westerly swell.......	Wind W.N.W.
" 28.	37 4 S. 10 47 E.	South-westerly swell.......	Wind S.W. to N.W.
1862. Jan. 2.	37 14 S. 31 18 E.	South-westerly swell.......	Calm on two previous days, wind S.W. to W.
" 9.	38 21 S. 60 45 E.	Westerly swell	Thrown off by cyclone experienced on 6th and 7th January.
" 10.	37 5 S. 62 54 E.	Westerly swell	Do. do.
" 10.	....	South-easterly swell	Thrown off from approaching cyclone.
" 13.	35 49 S. 69 36 E.	North-westerly swell.......	Thrown off from after part of cyclone.
" 18.	24 54 S. 71 59 E.	Swell from E.N.E..........	Wind N.N.E. and calm.
" 20.	21 30 S. 74 20 E.	Swell from E.N.E..........	Winds N.N.E. and N.E., with calms.
" 21.	19 10 S. 74 50 E.	Swell from E.N.E..........	Calm, and wind E.
" 22.	15 58 S. 75 15 E.	Easterly swell...............	Light winds E. to E.S.E.
" 23.	12 33 S. 76 3 E.	Easterly swell...............	S.E. wind and calm.
" 24.	10 3 S. 75 53 E.	South-easterly swell	Wind E. to E.N.E.
" 25.	7 53 S. 76 43 E.	S.S.E. swell	Light airs from N.E. and calm.
" 26.	6 6 S. 78 12 E.	Heavy swell from S.S.E.	Calm all day.
" 27.	3 58 S. 79 15 E.	S.S.E. swell.................	Wind North-westerly and calm.
" 28.	1 35 S. 80 13 E.	Westerly swell, confused with rollers from S.S.W.	Westerly swell, caused by Line westerly monsoon.
" 29.	1 21 N. 80 48 E.	Confused S.W. swell.	

Screw Steamer 'Thunder.'

Mar. 31.	11 33 N. 95 3 E.	Long swell from N.N.W.	Apparently caused by cyclone experienced at Sand-heads and head of Bay of Bengal on the 24th March.

Ship 'City of Pekin.'

1862. May 28.	14 25 N. 90 25 E.	S.S.-westerly swell..........	Caused by S.W. monsoon.
June 1.	9 28 90 19	South-westerly swell	Do. do.
" 2.	8 14 91 10	South-westerly swell	Do. do.
" 4.	7 12 89 56	South-westerly swell	Do. do.
" 9.	1 43 88 50	Southerly swell	Current N.E. by N. 49′.

Ship 'City of Pekin' (continued).

Date.	Ship's position.		Nature and direction of swell.	Remarks.
1862.	° ′	° ′		
June 11.	0 51 N.	88 29 E.	S.S.W. swell.	
" 12.	0 23	87 46	Swell from S.E. and S.S.E.	Wind S.S.E., light and calm.
" 13.	0 25 S.	86 35	Southerly swell, veering to S.S.E.	Wind S.S.E., light and calm.
" 14.	1 7	85 45	Southerly swell	Light winds from S.
" 16.	3 17	82 35	Southerly swell	S.E. Trade setting in.
" 17.	4 44	80 52	Southerly swell.	
" 19.	9 11	77 25	Heavy and long southerly swell.	Wind S.E. by S.
" 20.	11 40	74 35	Southerly swell	Wind S.S.E.
" 21.	13 27	72 0	Southerly swell	Wind S.S.E. and S.E.
" 22.	14 48	70 5	Southerly swell	Do. do.
" 23.	16 47	67 51	Southerly swell	Do. do.
" 26.	21 33	61 45	S.-westerly swell (18 feet).	Wind S.S.W., S., and S. by E.
" 27.	23 17	59 0	S.-westerly swell (20 feet).	
" 28.	23 57	55 32	S.-westerly swell, decreasing.	Wind E. to S.E., current W. 60′.
" 29.	24 47	53 56	Southerly swell	Wind from E., round by S. to W.
July 1.	27 13	50 23	North-easterly swell	Wind N.E., round by N. to W.S.W.
" 2.	28 57	47 46	South-westerly swell	Wind from N.W. to S. by W.
" 3.	29 7	44 16	South-westerly swell	Wind southerly.
" 7.	33 6	34 58	Southerly swell (16 feet).	Wind from eastward.
" 8.	33 17	32 45	Swell from E.N.E	Light winds from E.N.E. and E., and calms.
" 9.	33 23	31 28	Low westerly swell	Winds round the compass.
" 10.	33 43	28 48	South-westerly swell	Current S. 71°, W. 48′.
" 11.	35 3	25 14	W.S.W. swell.	
" 11.	8 p.m.		Heavy swell from W.	
" 12.	35 37	23 32	W.S.W. swell.	
" 13.	2 a.m.		Heavy westerly swell.	
" 14.	34 53	21 56	S.-westerly swell (16 feet).	Wind variable, and N.W. by W.
" 17.	35 6	19 10	Confused southerly swell.	Wind Southerly, S.S.E.
" 23.	24 5	5 20	Low southerly swell	Wind S.S.E.
" 24.	22 21	3 17	Southerly swell (11 feet).	Wind S.E.
" 25.	20 41	1 15	Southerly swell	Wind E.S.E.
" 26.	19 56	0 0	Low swell from E.S.E.	Wind S.E., light airs.
" 27.	19 44	0 18 W.	Low cross swell from E.S.E. and S.S.E.	Calm weather.
" 28.	19 17	0 40	Long south-westerly swell, at first low, but increasing.	Calm and light airs from SW.–S.S.E.
" 29.	18 0	2 37	Southerly swell	Moderate breeze at S.E.
" 30.	16 37	4 56	Southerly swell; confused swell during afternoon, rolling up from S.S.E.	Wind E.S.E. (S.E. Trade).
" 31.	14 54	7 23	South-easterly swell.	

Ship 'City of Pekin' (continued).

Date.	Ship's position.	Nature and direction of swell.	Remarks.
1862. Aug. 1.	13° 14′ S. 9° 10′ W.	Swell during day from S.E. and E.S.E. In the evening long and heavy from S.	
" 8.	2 51 19 20	Heavy swell, rolling up from S.	Wind S.E.
" 9.	0 55 21 0	Swell from S.S.W. to S.S.E. (10–12 feet).	Do. do.
" 10.	1 15 N. 22 38	Southerly swell (10 feet).	
" 11.	3 12 24 18	Southerly swell (16–18 ft.)	Do. do.
" 12.	4 57 25 45	Southerly swell (16 feet)	Light airs and calm.
" 13.	6 9 26 17	Heavy swell from S.S.E.	
" 14.	5 41 25 43	S.S.-easterly swell and ripples.	Calm and light airs from N.W.
" 15.	6 19	Low swell from S.	Calms.

In the above Table will be found instances of swells arising from prevalent or straight-lined winds, as the monsoons and trades, of those caused by currents, and of those thrown off by the passage of cyclones.

The swell from the southward and S.S.E. experienced in the Indian Ocean, in the region of the S.E. Trades, is at first sight rather puzzling.

The true explanation of the causes of these seems to be, that in the region of the S.E. Trades in that ocean, although the great mass of water is carried by the action of the prevailing winds towards the N.W., yet there exist from time to time currents running to the southward. No such currents are laid down upon any of our current-maps, yet I am persuaded that they exist in nature. An instance of this is afforded by the log of the ship 'City of Pekin,' on her outward passage to Calcutta, in the spring of the year 1862, from which the following notes are extracted:—

12th March.	35° 47′ S.	79° 10′ E.	Current South 27′
15th "	28 22	81 13	" South 27′
16th "	26 42	81 45	" South 23′
17th "	23 38	83 10	" S. 50°, E. 42′.

The S.E. Trade was fallen in with by this ship, on this passage, on the 15th March, in latitude 28° 22′ S., longitude 81° 13′ E.

The above are very remarkable observations. The tendency of the wind, and the ordinary drift of the surface-water, would conspire to place the ship ahead of her reckoning instead of astern of it; while the regularity in speed of the current observed is a strong argument in favour of their correctness.

It should also be noticed that the swell caused by prevailing winds often rolls home on a coast to which such winds do not extend. The most familiar instance of this is afforded by the westerly swell which is prevalent upon the coast of Portugal, although the westerly winds which cause it are separated from the coast by a tract occupied by winds which blow parallel to the coast and are called by sailors the Portuguese Trades. This peculiarity is well worthy of the attention of the student of physical geography, as having an important bearing upon the consideration of the abrasion of coasts.

From what has been above stated, I hope that it will be seen that this subject, although not usually considered worthy of attention and study, is of interest and importance. To the scientific traveller it gives hints of agencies being at work which, without it, would have been unsuspected, and in districts little explored indicates either the direction of prevailing winds or the set of currents; while to the seaman the cyclone swell gives timely notice of impending danger, and of the position of his enemy. Each of the classes of swell here mentioned has its own peculiar character and appearance, not easily explained in words, but from which an experienced eye can almost at once detect its cause.

XX.—*Notes on the Introduction of Steam Navigation.* By J. C. Dyer, Esq.

Read February 10th, 1863.

"Whatever saves labour, rewards labour."—Governor Morris.

The application of steam power to propel boats and ships being a subject of great public interest, has from time to time been treated by many able writers advocating the claims of the different parties alleged to have been the first inventors of the means of using this power to supersede that of the wind for propelling ships. Some of these writers have given a national importance to the questions of originality among the experimenters who claimed priority in the different parts of Europe and America, where trials had been made of their several schemes with various results. On these results, and their subsequent influence on steam navigation, many sharp controversies formerly appeared; but of late years these seem to have subsided into the quiet assumption, on behalf of each nation, that its claimants were fairly entitled to the honour of having been the first discoverers of steam navigation. According with this impression, two letters have appeared in 'The Times' respecting the "first introduction of steamers into the English waters;" the first of which was copied from the 'Dumbarton Herald,' and the second, in reply thereto, is signed "Investigator," whose statements of the facts of the case are given in 'The Engineer' of December 12th, 1862, thus:—"Seeing that there has been a discussion, and that there still remains an uncertainty as to who has the right to claim the honour of placing the first steam-ship in English waters, I beg to submit the following statement of authentic facts for settling the

matters in dispute. The 'Margery' was built at Dumbarton by the late Mr. William Denny, for Mr. W. Anderson, merchant, Glasgow, and when launched was christened the 'Margery,' after his eldest daughter, who named her, who is still alive, and a resident in London. At the close of the year 1814, Captain Curtis was sent by a London Company to Glasgow to negotiate with Mr. Anderson for the purchase of the 'Margery,' which was effected, the only stipulation made by Mr. Anderson being that the name of the steamer should at no future period be changed; this Captain Curtis agreed to, and the promise was faithfully kept. Captain Curtis took the 'Margery' through the Forth and Clyde Canal, and invited a large party of Mr. Anderson's friends to accompany him while passing through the canal. There remain but two of this party now alive, viz. the lady after whom the steamer was named, and a clergyman a friend of Mr. Anderson's. The writer of the article in the 'Dumbarton Herald' is quite correct in his statement of the fear and wonder which the appearance of the 'Margery' excited on the coast while on her passage to England, as well as among the English fleet; in most cases she was supposed to be a vessel on fire. The 'Margery' was the first steam-ship that ever sailed in English waters, and made her first trip to Milton, below Gravesend, on the 23rd January 1815. She was ultimately taken to Paris, where not many years ago her timbers were still lying on the banks of the Seine. Mr. Anderson was therefore owner of the first steamer that was ever seen in London, and also the first in Paris. He also owned the first that ever crossed from Scotland to Ireland (namely the 'Greenock,' built soon after the 'Margery'), which he took to Belfast."

Considering that fifty-five years have passed since the first successful application of steam power to navigation was clearly established, and witnessed by myriads of people

at New York and on the Hudson River, we may reasonably invoke a calm review of the steps taken by the author of that success, as well as of those who had been engaged in the pursuits of employing steamers in Europe and America.

The first steam-boat established as a packet for passengers between New York and Albany was the 'Claremont,' built in 1806, and launched in the spring of 1807, and continued to run during the remainder of that year. As it was not until 1815 that the first steamer was seen in English waters, the successful application of steam to navigation was therefore *eight years* sooner in the American waters; and the honour of that success can hardly be denied to Robert Fulton, who achieved it, and whose preceding labours had gradually led him to its accomplishment. I propose to notice a few of Mr. Fulton's previous experiments and speculations upon the subject, without at all calling in question the merits of other ingenious men engaged in the same inquiries, though none of them had succeeded in practical steam navigation, so that either by the turn of fortune, or by the exercise of superior judgment and skill, Robert Fulton is justly entitled to rank as the author of steam navigation; and when the above facts are fairly considered, I doubt not that the English people will willingly accord the meed of praise due to him for the genius that conceived, and the persevering labour that led to his triumphant command of the elements, that enable us now "to walk over the oceans in the midst of their stormy terrors."

In the year 1793 Mr. Fulton communicated his scheme for navigating by steam to Lord Stanhope, and received his lordship's thanks for the same, in September of that year. In 1811 I communicated with his lordship on the subject of bringing into use in England Mr. Fulton's inventions for steam navigation. Lord Stanhope then con-

firmed to me the fact of his having received Mr. Fulton's plans so early as 1793, and of his having conferred with him upon their practical application. In 1803 Mr. Fulton constructed a steam-boat on the Seine, which satisfied him of the correctness of the principle he had adopted; and in conjunction with the American Minister, Mr. Livingston, it was determined to transfer their joint exertions for establishing steam navigation to the American waters, for which purpose a steam-engine was ordered from Messrs. Boulton and Watt. From various causes of delay, Mr. Fulton did not arrive in New York until 1806. During that year he devoted his attention to superintend the building of the 'Claremont' in the shipyard of Mr. Charles Brown. This vessel was 133 feet long, 18 feet beam, and 160 tons burden, and was employed, as aforesaid, in the summer of 1807.

I have sailed in this vessel in company with Mr. Fulton, and retain a vivid recollection of the general interest which this great enterprise excited, and of the admiration bestowed upon its author, even by the many persons who had shortly before ridiculed his projects as chimerical.

It is not my present purpose to join issue in any of the discussions concerning the original application of steam power to navigation, the subject having been exhausted by the respective advocates claiming it on behalf of England, France, Switzerland, and America. I content myself with stating the simple fact, that all of the experiments in each country, which preceded those of Mr. Fulton, had already proved, without any exception, utter failures, and no benefit whatever had arisen from the application of any one of the trials to navigate by steam prior to the complete success of the 'Claremont' packet in the summer of 1807, on the Hudson River.

It is worthy of remark, that the sensations of astonishment and alarm, among the spectators on shore and the crews of the vessels, created by the 'Claremont' in 1807,

were exactly the same as those created by the 'Margery' among the vessels on the Thames in 1815, or eight years afterwards; this will be seen by Mr. Colden's description of the 'Claremont's' first voyage, and Mr. Anderson's account of the first voyage of the 'Margery,' as before given.

Steam could not be successfully employed to give rotatory motion to machinery by any of "the inventors of steam-engines," before the great improvements brought into use by James Watt. Considering that steam power had not been made to supersede water-wheels and horses, for giving rotatory motion to fixed machines on land, it was certain to fail as applied to such motion for propelling ships. It is needless, then, to notice any of the several schemes that had been proposed, or tried, for steam navigation, except those based on the use of Watt's steam-engine; and all inquiry concerning these are of interest only as they unfold the approaches to success attained by the several claimants, before the actual success of Robert Fulton in 1807. It will suffice, then, shortly to mention the several methods employed by the persons claiming to have been the "inventors of steam navigation."

In France, the Marquis de Jouffroy claims to have constructed a steam-boat with paddle-wheels at Lyons in 1782, which, however, was not heard of until 1816 (thirty-four years afterwards), when the first boat on Fulton's plan was started on the Seine; and *then* the Marquis complained loudly of Fulton's boat as being a piracy of his invention. On this occasion, Monsieur Royou (in the 'Journal des Débats,' 16th March 1816), in reply to the Marquis, says, "It is not concerning an invention, but the means of applying a power already known. Fulton never pretended to be an inventor, in regard to steam-boats, in any other sense. The application of steam to navigation had been thought of by all artists; but the means of executing it were wanting, and Fulton furnished them."

Dr. Franklin, in 1785, writes to Monsieur Alphonse Leroy thus:—"Several projectors have at different times proposed to give motion to boats, and even to ships, by paddles placed on the circumference of wheels on each side of the vessel; but this method has been found so ineffectual, as to discourage a continuance of the practice"*.

The plan proposed by Daniel Bernoulli, in 1783, was by driving a column of water out at the stern of the vessel; which plan has been many times suggested, and several times tried by other ingenious men, but without success. It seems strange that, to so eminent a mathematician as Bernoulli, the radical defects of this plan should not have occurred. As the water issues from the mouth of the tube, it escapes in the radial lines of a semisphere. The resisting forces will be directly as the distance of each of the radii from the surface, and their propelling power will be equal to the force with which the water is driven from the orifice, *only* in the direct line of the tube's centre, and it will diminish with the angular deviation of the radii from that line, until it becomes nil at right angles; wherefore this mode of pressing water against water (though simple and plausible at first sight) is the most wasteful expenditure of propelling force of any that has been proposed.

It appears that "endless-chain floats" have been many times proposed and patented; but this plan, too, is defective in principle, and has always failed in practice. The chain-floats are driven horizontally, and successively acting upon the same column of water, generate a current in the direction of their motion, and much of the propelling power is lost by moving and agitating the water. In an experiment I witnessed in 1813 (in a boat on the Bridgewater Canal), the floats were placed about four feet apart, and when first started, the boat moved with considerable speed; but as the speed of the floats increased,

* Life of Dr. Franklin, vol. iii. p. 528. London, 1818.

that of the boat decreased. Then every other float was removed, and at a new start better speed was obtained, but could not be kept above three miles the hour. Then all the floats were removed, and the chain only dragged through the water; this carried the boat a trifle faster than the floats had done.

In 1795 Lord Stanhope made experiments with a steam-boat with the "duck's-foot paddles," which did not succeed. The defects of this form of propelling arise from the loss of time in withdrawing the paddle between each propulsion, and in the waste of power in this retrograde motion.

In 1785, James Rumsay, of Virginia, constructed a steam-boat, which was tried on the Potomac in 1787, and which sailed by means of steam four miles an hour, as stated in Dr. Rush's letter to Dr. Letsome; but the boat was not continued on the Potomac, and Rumsay afterwards tried his plan in London without success. About the same time, Mr. Fitch of Philadelphia made experiments on the Delaware River for propelling boats by paddle-wheels; but, owing to his miscalculation of the propelling-wheels, and of the steam-power as applied to the resistances to be overcome, his boats did not succeed, and were given up as failures, but were revived as his invention after the success of the 'Claremont.'

J. C. Stephens, of New York, in 1804 made experiments with a steam-boat 25 feet long and 5 feet wide; engine cylinder $4\frac{1}{2}$ inches diameter, with 9-inch stroke. At first she broke her steam-pipe; but after repairs she ran for a fortnight on the Hudson River, making two or three miles an hour, crossing from Hoboken to New York: therefore it is said by a distinguished writer, "Mr. Stephens has the merit of being the first person who took a steam-boat to sea." (Qy. Did he take this boat to sea on board of another vessel?)

In 1788 and 1789, William Symington, in conjunction

with Patrick Miller and James Taylor, made several experiments on patents they had obtained relating to steam navigation, and in 1802 started a boat on the canal at Glasgow, which ran at the rate of three miles an hour, and it was concluded that his plan would supersede horses in canal navigation. The wheel was placed at the stern of the boat; but he states that the wheel, or wheels, may be at the sides if preferred. The boat, however, was discontinued, and no more was heard of Symington's boats until long after those of Fulton had become widely extended on the American waters.

The first ocean steamer was the 'Fulton,' of 327 tons, built in 1813 by A. and N. Brown at New York. The first steamer constructed for harbour defence, under the personal superintendence of Mr. Fulton, was built in 1814, of 2470 tons burden. This boat has been the type from which all the iron-clad batteries and rams have since been constructed, with various modifications, by later inventors.

Thus it appears that the continuous rotative motion of the paddle-wheel and the screw propellers are the only means yet discovered for navigating by steam-power with safety and effect.

In the specifications of Mr. Fulton's inventions, he gives drawings and descriptions—(1) of the chain-float; (2) of the duck's-foot paddle; (3) of the screw, fan, or smoke-jack propeller; and (4) of his paddle-wheels; with which several plans he had made experiments in France, which led him to throw aside the three first, and to adopt the paddle-wheel as the best in practice according to the then powers of construction; for it is well known that it was many years after the first screw steamer was constructed (the 'James Watt,' running from London to Havre) before a safe screw propeller could be made, for large ships, equal to the paddle-wheels.

Having witnessed the triumphant success of Fulton's

steam-boats on the Hudson River, and their rapid increase for navigating the other American rivers, I undertook, in 1811, the task of inducing some of the leading engineers and capitalists of London to engage in the introduction of steam-boats, on Fulton's plan, to run on the Thames and other waters in this country. Believing that they must soon be adopted and become of great importance to England, as they were so rapidly becoming in the United States, I had obtained from Mr. Fulton (through a mutual friend) a full description and the drawings of his inventions and discoveries relating to steam navigation, with the result of his labours in America. But I found it impossible to convince any of them that steam-boats could be made to run with safety and profit in the English waters. The general reply was, "We don't doubt the success of steam-boats in the large American rivers and inlets from the sea, but they will never answer in our (comparatively) small rivers and crowded harbours."

Many of my personal friends urged me strongly not to waste my time and money on so hopeless a task as that of introducing steam navigation into England. Even the great and scientific engineer, John Rennie (father of the present eminent Sir John Rennie), urged me, with parental kindness, to drop all thoughts of bringing these boats into use—and this after having Fulton's plans before him, and fully admitting their success in America. Thus we see how difficult it is to make even great men move in any path before the destined time. Our late distinguished townsman, Peter Ewart, Vice-President of this Society, dissuaded me, as a personal friend, from trying to introduce steam-boats into England, saying that "he knew of the trials made here without success, as also of those in America which were successful; but it did not appear likely that they could ever come into general use in the waters of England." This opinion of Mr. Ewart was expressed in the

spring of 1814, just a year before the 'Margery' was passed through the canal from the Clyde to the Forth, to make her first voyage in the English waters, as before stated. Mr. Ewart was fully informed of the nature and the results of the trials of the small boat constructed by John Bell, and run a short time, in the autumn of 1813 and the spring of 1814, on the Clyde and Forth before she was finally discontinued as a failure, which experiment had no tendency to convince him, any more than other English engineers, of the practical utility of steam navigation in English waters. In that year (1814) I lent Mr. Ewart Fulton's specifications and drawings, which were sent by him to Boulton and Watt, and returned to me about six months after. I have reason to believe that that eminent house was led thereby to make further and more exact inquiries concerning the progress of steam navigation in America; for they, as well as several other engineers, commenced building steam-boats in 1815 and 1816, since which time the progress of steam navigation has been marvellous for the perfection and the extension of British-built steamers both for inland navigation and, finally, for traversing alike the narrow seas and "the broad oceans that belt the globe."

The engineering talent, the mechanical skill, and the active enterprise that abounded in England had created a self-reliance which seemed to forbid the direction of either into other channels than those marked out at home. Her most gifted men were satisfied with the progress of knowledge *within* the realm. National intercourse was then both irregular and sluggish; so that peoples were to each other real strangers, and much given to mutual jealousies. These recollections serve to explain the fact that eight years had passed away from the time when the waters of the Hudson were first agitated by the paddles of the 'Claremont,' and when over 5000 tons had been

launched upon her bosom, before those of the Thames welcomed those of the 'Margery' steamer. The desire for instruction ever lags far behind the means of imparting it; hence the slow pace of nations in gaining knowledge through reports of its spread in other lands. This dislike to the "search for teachers" is alike found among men individually and in their national aggregates—all presenting the type of "the whining schoolboy, with his satchel and shining morning face, creeping like snail unwillingly to school."

At length, however, successful teachers have raised the spirit of lofty enterprise; and, by reason of extended and personal intercourse, relations of mutual benefit have been so widely extended that peoples of different nations begin to approach the condition of a vast cooperative society, giving to each member the utmost value of their joint labours.

Towards this benign end, steam-power has been the most important and effective agent. To bring this power under control, and render it both safe and economical in practice, first, for driving "labour-saving machinery" in our workshops and perfecting our manufactures, secondly for transporting goods and passengers over the rivers and oceans, and thirdly for the safe and rapid transit of them over land on iron rails, the names of Watt, Fulton, and Stephenson stand foremost as the great men whose genius and science made each of them a successful pioneer in the march of those grand objects.

The theory and practice of mechanical science have advanced with such rapid strides in our times as to form a leading feature in the progress of nations; and the records of these should rest on clearly ascertained facts, so that each leading contributor shall receive his due meed of fame, just as they claim to occupy a niche in her temple. This award will finally be made, apart from the question

of their respective nationalities. I have therefore aimed to explain fairly, and in due order of time, the several attempts made to introduce steam navigation, which led to success in the hands of Fulton in 1807.

In looking back to the many inventions of steam-engines that preceded the grand success of James Watt, it will be seen that the nature of his discoveries, as applied to the steam-engine, was very analogous to that of Fulton's as applied to steam navigation. The one was the first to render the steam-engine of great *practical utility,* the other was the first to render steam navigation practically safe and useful. These simple facts exhibit the puerile vanity of striving to erect national trophies upon the unaided labours of eminent men. Inventions and discoveries are made by individuals, not by nations; let each inventor, then, have his name honoured in the proportion that his labours have proved beneficial among nations. Considering that inventions do not spring into existence in perfection from their birth, like Pallas from the brain of Jupiter, but come from the previous labours of many brains, he is the real inventor who first gives vitality to those labours. In this sense the "invention of steam navigation" will for ever illustrate the mane of Robert Fulton.

Fulton's Steam-boat, the 'Claremont,' on the Hudson River.

XXI.—*On the Solution of the Differential Resolvent.* By W. H. L. Russell, A.B. Communicated by the Rev. Robert Harley, F.R.A.S.

Read March 11th, 1863.

In the course of last year I was induced, at the request of Mr. Harley, to consider the very interesting differential equation which he has denominated the "Differential Resolvent." I obtained the solution of the quartic resolvent by series which I summed by means of a triple integral. But Professor Boole intimated that he had discovered a process of transformation by which the quartic could be solved by a single integral. This led me to examine my own series, and I found that the series representing the solution of the quartic could be summed by means of a single integral. I have since discovered that the general resolvent can be solved by means of a single integral. To effect this is the object of the present paper.

As the investigations are complicated, I shall first, to fix the ideas, give the solution of the quartic.

The quartic resolvent is the equation

$$y-\frac{\left(\mathrm{D}-\frac{7}{4}\right)\left(\mathrm{D}-\frac{10}{4}\right)\left(\mathrm{D}-\frac{13}{4}\right)}{\mathrm{D}(\mathrm{D}-1)(\mathrm{D}-2)}\epsilon^{3\theta}y=0,$$

where $\epsilon^{\theta}=x$.

I have already expressed the solution of this equation in series, which will be found in the 'Proceedings' of the Manchester Society. One of these series is the following:—

$$1+\frac{\frac{13}{4}\cdot\frac{10}{4}\cdot\frac{7}{4}}{5\cdot4\cdot3}x^3+\frac{\left(\frac{25}{4}\cdot\frac{13}{4}\right)\left(\frac{22}{4}\cdot\frac{10}{4}\right)\left(\frac{19}{4}\cdot\frac{7}{4}\right)}{(8\cdot5)\cdot(7\cdot4)\cdot(6\cdot3)}x^6+.$$

The general term of this is

$$\frac{\left(\frac{13}{4}\cdot\frac{25}{4}\cdot\frac{37}{4}\cdots\frac{12m+1}{4}\right)}{5\cdot 8\ldots\ldots 3m+2}\cdot\frac{\left(\frac{10}{4}\cdot\frac{22}{4}\cdot\frac{34}{4}\cdots\frac{12m-2}{4}\right)}{4\cdot 7\cdot 10\ldots\ldots 3m+1}$$

$$\frac{\left(\frac{7}{4}\cdot\frac{19}{4}\cdot\frac{31}{4}\cdots\frac{12m-5}{4}\right)}{3\cdot 6\cdot 9\ldots 3m}x^{3m}\ldots$$

The reader who will attentively examine this expression, will see that it can be transformed into

$$\frac{\frac{7}{4}\cdot\frac{10}{4}\cdot\frac{13}{4}\cdot\frac{16}{4}\cdot\frac{19}{4}\cdot\frac{22}{4}\cdot\frac{25}{4}\cdot\frac{28}{4}\cdots\frac{12m+1}{4}}{(3\cdot 4\cdot 5\ldots 3m+2)\cdot(4\cdot 7\cdot 10\ldots 3m-2)}x^{3m}$$

$$= Cx^{3m}\cdot\frac{\left(\frac{3}{4}\right)^{4m-1}\cdot\Gamma\left\{4m+\frac{4}{3}\right\}}{3^{m-1}\cdot\Gamma(3m+3)\cdot\Gamma\left(m+\frac{1}{3}\right)}$$

(where C is a certain irrelevant factor)

$$= \frac{C}{x^2}\int dx\,.\,x^{3m+1}\,.\,\frac{3^{3m}}{4^{4m}}\cdot\frac{\Gamma\left\{4m+\frac{4}{3}\right\}}{\Gamma(3m+2)\Gamma\left(m+\frac{1}{3}\right)}.$$

Now we know that

$$\int_{-\frac{\pi}{2}}^{\frac{\pi}{2}}\cos^{a+b-2}\theta\,.\,\epsilon^{(a-b)i\theta} = \frac{\pi}{2^{a+b-2}}\cdot\frac{\Gamma(a+b-1)}{\Gamma a\Gamma b}.$$

Hence we shall have

$$\frac{\Gamma\{4m+\frac{4}{3}\}}{\Gamma(3m+2)\,\Gamma(m+\frac{1}{3})} = \frac{2^{4m+\frac{1}{3}}}{\pi}\int_{-\frac{\pi}{2}}^{\frac{\pi}{2}} d\theta\,\cos^{4m+\frac{1}{3}}\theta\,\epsilon^{(2m+\frac{5}{3})i\theta},$$

whence we obtain as the sum of the series,

$$\frac{C}{x^2}\int dxx\int_{-\frac{\pi}{2}}^{\frac{\pi}{2}} d\theta\frac{\cos^{\frac{1}{3}}\theta\left\{\cos\frac{5\theta}{3}-\cos^4\theta\cos\frac{\theta}{3}\cdot\frac{(3x)^3}{2^4}\right\}}{1-\cos^4\theta\cos 2\theta\left(\frac{3x}{2}\right)^3+\cos^8\theta\frac{(3x)^6}{2^8}}.$$

I now proceed to the solution of the general equation,

$$y-\phi(\mathrm{D})\epsilon^{(n-1)\theta}y=0,$$

where

$$\phi(\mathrm{D})=\frac{\left(\mathrm{D}-\frac{2n-1}{n}\right)\left(\mathrm{D}-\frac{3n-2}{n}\right)\dots\left(\mathrm{D}-\frac{n^2-n+1}{n}\right)}{\mathrm{D}(\mathrm{D}-1)(\mathrm{D}-2)\dots(\mathrm{D}-n+2)}$$

The solution of this equation may be expressed in series thus:

$$\begin{aligned} y=&\,\mathrm{C}_0\{\mathrm{A}_0^{(0)}+\mathrm{A}_1^{(0)}x^{n-1}+\mathrm{A}_2^{(0)}x^{2(n-1)}+\dots\} \\ &+\mathrm{C}_1x\{\mathrm{A}_1^{(1)}+\mathrm{A}_1^{(1)}x^{n-1}+\mathrm{A}_2^{(1)}x^{2(n-1)}+\dots\} \\ &+\mathrm{C}_2x^2\{\mathrm{A}_0^{(2)}+\mathrm{A}_1^{(2)}x^{n-1}+\mathrm{A}_2^{(2)}x^{2(n-1)}+\dots\} \\ &+\ \&c. \\ &+\mathrm{C}_rx^r\{\mathrm{A}_0^{(r)}+\mathrm{A}_1^{(r)}x^{n-1}+\dots\mathrm{A}_m^{(1)}x^{m(r-1)}+\dots\}, \end{aligned}$$

where $\mathrm{A}_m^{(r)}=$

$$\frac{\left(r+(n-1)-\frac{2n-1}{n}\right)\left(r+2(n-1)-\frac{2n-1}{n}\right)\dots\left(r+m(n-1)-\frac{2n-1}{n}\right)}{\left(r+(n-1)\right)\left(r+2(n-1)\right)\dots\dots\left(r+m(n-1)\right)}.$$

$$\frac{\left(r+(n-1)-\frac{3n-2}{n}\right)\left(r+2(n-1)-\frac{3n-2}{n}\right)\dots\left(r+m(n-1)-\frac{3n-2}{n}\right)}{\left(r+(n-2)\right)\left(r+(n-2)+(n-1)\right)\dots\left(r+(n-2)+(m-1)(n-1)\right)}$$

$$\frac{\left(r+(n-1)-\frac{4n-3}{n}\right)\left(r+2(n-1)-\frac{4n-3}{n}\right)\dots\left(r+m(n-1)-\frac{4n-3}{n}\right)}{\left(r+(n-3)\right)\left(r+(n-3)+(n-1)\right)\dots\left(r+(n-3)+(m-1)(n-1)\right)}$$

. . . .

$$\frac{\left(r+(n-1)-\frac{n^2-n+1}{n}\right)\left(r+2(n-1)-\frac{n^2-n+1}{n}\right)\dots\left(r+m(n-1)-\frac{n^2-n+1}{n}\right)}{(r+1)\left((r+1)+(n-1)\right)\dots\left(r+1+(m-1)(n-1)\right)}.$$

This term can be transformed by a method similar to that which we employed for the quartic resolvent, and we find this expression equivalent to

$$\frac{\left(r-\frac{1}{n}\right)\left(r+\frac{n-2}{n}\right)\left(r+\frac{2n-3}{n}\right)\dots\left(r+m(n-1)-\frac{2n-1}{n}\right)}{\{(r+1)\dots r+m(n-1)\}\{(r+m(n-1)-n)(r+(m-1)(n-1)-n)\dots(r+n-2)\}}$$

$$=\mathrm{C}\,.\,\frac{(n-1)^{(n-1)m}}{n^{nm}}\,.\,\frac{\Gamma\left\{nm+\frac{(r-1)n}{n-1}\right\}}{\Gamma\left\{m+\frac{r-1}{n-1}\right\}\Gamma\left\{r+m(n-1)+1\right\}}.$$

Hence the general term may be written

$$\frac{C}{x^r}\int dx x^{r-1}\frac{(n-1)^{(n-1)m}}{n^m}\cdot\frac{\Gamma\left\{nm+\frac{(r-1)n}{n-1}\right\}}{\Gamma\left\{r+m(n-1)\right\}\Gamma\left\{m+\frac{r-1}{n-1}\right\}}x^{m(n-1)}.$$

Now

$$\frac{\Gamma\left\{nm+\frac{(r-1)n}{n-1}\right\}}{\Gamma\left\{r+m(n-1)\right\}\Gamma\left\{m+\frac{r-1}{n-1}\right\}}$$

$$=\frac{2^{nm+\frac{(r-1)n}{n-1}-1}}{\pi}\int_{-\frac{\pi}{2}}^{\frac{\pi}{2}} d\theta\ \cos^{nm+\frac{(r-1)n-1}{n-1}}\theta\,\epsilon^{\left(m(n-2)+r-\frac{r-1}{n-1}\right)i\theta}$$

whence we find the sum of the series

$$\frac{C_r}{x^r}\int dx x^{r-1}\int_{-\frac{\pi}{2}}^{\frac{\pi}{2}} d\theta\ \cos^{\frac{nr-2n+1}{n-1}}\theta$$

$$\frac{\cos\frac{nr-2r+1}{n-1}\theta-\frac{2^n(n-1)^{n-1}}{n^n}\cos^n\theta\cos\left\{n-2-\frac{nr-2r+1}{n-1}\right\}\theta x^{n-1}}{1-\frac{2^{n+1}(n-1)^{n-1}}{n^n}\cos^n\theta\cos(n-2)\theta x^{n-1}+\frac{2^{2n}(n-1)^{2(n-1)}}{n^{2n}}\cos^{2n}\theta x^{2n-2}}.$$

By giving different values to (r), we obtain particular integrals of the equation in succession. It is evident from this investigation that the multiple integrals by which I had previously expressed the solution of the differential equation may be reduced to single integrals. In effecting this we must, of course, carefully restore the factors omitted in the transformations given in this paper.

Note.—To make this communication more complete, I here insert the methods by which the series employed were derived from the equations. The following rule to obtain the series which express the solution of linear differential equations when in the symbolical form, is extracted from

Professor Boole's 'Treatise on Differential Equations,' page 427.

"If a linear differential equation, whose second member is zero, be reduced to the symbolical form

$$f_0(\mathrm{D})u+f_1(\mathrm{D})\epsilon^{\theta}u+f_2(\mathrm{D})\epsilon^{2\theta}u+\ldots+f_n(\mathrm{D})\epsilon^{n\theta}u=0,$$

then a particular solution will be $u=\Sigma u_m\epsilon^{m\theta}$, the value of the index (m) in the first term being any root of the equation $f_0(m)=0$, the corresponding value of u_m an arbitrary constant, and the law of the succeeding constants being expressed by the equation

$$f_0(m)u_m+f_1(m)u_{m-1}+f_2(m)u_{m-2}+\ldots+f_n(m)u_{m-n}=0."$$

This rule is proved immediately by substituting the series as the value of (u) in the above differential equation. Let us apply this to the quartic resolvent

$$y-\frac{\left(\mathrm{D}-\frac{7}{4}\right)\left(\mathrm{D}-\frac{10}{4}\right)\left(\mathrm{D}-\frac{13}{4}\right)}{\mathrm{D}(\mathrm{D}-1)(\mathrm{D}-2)}\epsilon^{3\theta}y=0,$$

derived from the algebraical equation $y^4-4y+3x=0$.

The equation in (m) given above becomes

$$m(m-1)(m-2)u_m-\left(m-\frac{7}{4}\right)\left(m-\frac{10}{4}\right)\left(m-\frac{13}{4}\right)u_{m-3}=0,$$

and the equation $f_0(m)=0$ becomes $m(m-1)(m-2)=0$.

Taking the root $m=2$ as the initial value, we determine the coefficients of the series in succession by putting

$$m=5,\ 8,\ 11\ u\ldots$$

and we have

$$5\cdot4\cdot3\cdot u_5=\frac{13}{4}\cdot\frac{10}{4}\cdot\frac{7}{4}u_2$$

$$8\cdot7\cdot6\cdot u_8=\frac{25}{4}\cdot\frac{22}{4}\cdot\frac{19}{4}u_5\ldots;$$

$$\therefore u_5=\frac{\frac{13}{4}\cdot\frac{10}{4}\cdot\frac{7}{4},}{5\cdot4\cdot3}$$

$$u_8 = \frac{\left(\frac{25}{4} \cdot \frac{13}{4}\right)\left(\frac{22}{4} \cdot \frac{10}{4}\right)\left(\frac{19}{4} \cdot \frac{7}{4}\right)}{(8 \cdot 5) \cdot (7 \cdot 4) \cdot (6 \cdot 3)},$$

and the series which results is as follows:

$$Cx^2 \left\{ 1 + \frac{\frac{13}{4} \cdot \frac{10}{4} \cdot \frac{7}{4}}{5 \cdot 4 \cdot 3} x^3 + \frac{\left(\frac{25}{4} \cdot \frac{13}{4}\right)\left(\frac{22}{4} \cdot \frac{10}{4}\right)\left(\frac{19}{4} \cdot \frac{7}{4}\right)}{(8 \cdot 5) \cdot (7 \cdot 4) \cdot (6 \cdot 3)} x^6 + \ldots \right\},$$

which is the same as that we have employed above. The reader will now have no difficulty in deducing the series in the general case.

XXII.—*Note as to two Events in the History of Steam Navigation.* By W. J. Macquorn Rankine, C.E., LL.D, F.R.S., Hon. Mem. of the Literary and Philosophical Society of Manchester.

Read April 7th, 1863.

1. An interesting paper was lately read to this Society by Mr. Dyer, containing a history of a series of important events in the progress of steam navigation.

2. It is to be regretted, however, that the author has noted either very slightly, or not at all, what appears to have been an event of paramount importance in the first adaptation of the double-acting cranked steam-engine to drive a paddle-wheel. Before that adaptation was made, the success of all attempts at steam navigation, such as those of Jouffroy, Rumsay, Fitch, Miller, Taylor, &c., had been only temporary, because of the rudeness of the machinery for communicating motion from the piston to the shaft.

3. That first adaptation was unquestionably accom-

plished by William Symington in 1801, as is proved by authentic documents, which have been published by Mr. Woodcroft in his 'Origin and Progress of Steam Navigation.' Symington, instructed by the failure of the ratchet-work engine which he had made for Miller's boat, fitted up the 'Charlotte Dundas,' in 1801, with a double-acting horizontal cranked engine, and thus made her what Mr. Woodcroft has justly called "the first practical steam-boat." Her speed, when running alone, and not towing other boats, was six miles an hour.

4. The use of this vessel was abandoned, not from any fault in her construction or working, but because the Directors of the Forth and Clyde Canal feared that she would damage its banks. Yet the man in all Britain who possessed at that time the greatest practical experience of the working of canals (the Duke of Bridgewater), was not deterred by any such apprehension from ordering, in 1802, *eight* similar vessels from Symington to be used on his canal.

5. The death of the Duke of Bridgewater early in the following year prevented the execution of that order. But Symington had evidently done all that lay in his power, and all that was necessary, to convert the steam-boat from an awkward piece of experimental apparatus to a practically useful machine; and the honour paid to his memory ought not to be lessened because the career of his invention was cut short by a misfortune.

6. There is nothing in this to detract from the honour which is justly paid to Fulton as having been the first to practise steam navigation on a great scale as a commercially profitable art.

7. Another event, passed over in the paper to which I have referred, is the first introduction of commercial steam navigation into Europe, which was effected on the River Clyde, in 1812, by Henry Bell, as is proved by documents cited in Mr. Woodcroft's work already referred to.

XXIII.—*On the Planet Mars.* By JAMES NASMYTH, Esq., C.E. In a Letter to JOSEPH SIDEBOTHAM, Esq.

Read March 24th, 1863.

DURING the months of September and October last, when the planet Mars was favourably situated for observation, I had, on two or three occasions, the good fortune to obtain some fine views of him.

Under the impression that a few remarks on the aspect of the planet may interest you and some of the Members of the Manchester Literary and Philosophical Society, I have sent you, along with this, a rough but faithful drawing (see Plate X.) of the aspect of the planet, as revealed to me by the aid of my 20-in. diameter reflecting telescope.

One of the most striking and interesting features was the patch of snow (?) situated near the south pole of the planet. I use the term "patch" as most expressive of its appearance. It was so distinct and definite as to appear like a white wafer laid on the pole of a globe; and what contributed much to this distinct and definite aspect was the remarkable contrast between its tint of pure white and a brown-grey tint in the parts immediately surrounding it. I have endeavoured, as carefully as I can, to represent this in the accompanying drawing. The "patch"-like aspect of this feature was enhanced by the impression of a cliff-like edge to it, which I have also endeavoured to convey in the drawing. The brilliant white of this south pole snow-patch, in contrast with the dull and ruddy tint of the rest of the planet contiguous to it, forcibly conveyed the impression that the patch in question was the *snow* of the south arctic pole, then in its summer position.

The snow on the north pole was also visible; but as the north arctic pole, then in its winter position, was turned away from our direct line of vision, it was not so well seen; and the manner in which the white of the snow on the north pole was blended or softened off into the ruddy tint of the surface of the planet at this part removed the sharp and definite boundary which characterized that of the snow-patch on the south arctic pole. I use the expression south *arctic* pole in contradistinction to that of the "south pole" for this reason, that I find the south arctic pole does not coincide with the absolute south pole of the planet; it is somewhat eccentric to it, and hence it is that in the rotation of the planet on its axis the snow-patch on the south pole went nearly out of sight when the planet turned half round. This observation is the more interesting as it tends to establish a similarity in that respect to the situation of *our* arctic poles, which are, I believe, known to be somewhat on one side of, or excentric to, the absolute poles of the earth. What I term the *arctic* pole is the centre of minimum temperature, which is influenced by the relative situation and area of land and water on our globe near its poles.

What next most attracted my attention was the remarkable and beautiful contrast between the ruddy portions of the surface of the planet and those which appeared of a pale blue-green tint, forcibly conveying the impression of the presence of land and sea. This was rendered the more striking by an isolated ruddy patch, as represented in the drawing, and which I could not resist the conclusion was *an island*!

I shall never forget the impression which those remarkable and beautiful features, presented by the planet Mars on this occasion, made upon me.

On comparing the aspect of the planet on the same occasion as revealed to me by the aid of my 20-in. diameter

reflecting telescope, as contrasted with the view furnished by the aid of a very fine 8-in. aperture achromatic telescope by Cooke, of York, I was much impressed with the superior distinctness with which the *tints* of the relative portions of the surface of the planet were brought out by the reflecting telescope as compared with the achromatic. Although the definition of the planet was as perfect as could be desired when seen by the latter instrument, the markings on the surface of the planet could only be distinguished by their variety (or difference) of relative *shade* or brightness, while in the view furnished by the reflector the actual tints or colours of the various features were rendered quite distinct. This was most prominently the case in respect to the blue-green of the supposed sea (?) and the ruddy tint of the land (?) and island (?). As before said, the snow-patch on the south pole was rendered peculiarly distinct and definite by the presence of a remarkable dark local shade immediately surrounding it. In all these respects the drawing is as faithful a representation as such a means can enable me to accomplish.

In respect to the cause of the superior manner in which the various tints of the features of the planet were rendered by the reflecting as compared with the achromatic telescope, I am disposed to assign it to the fact that in the case of the employment of a reflecting telescope the light from the planet suffers no change or decomposition in its passage to the eye. Although some of the light is lost by reflection, yet the integrity of its original composition is maintained, and it reaches the eye of the observer in its original virgin state; whereas, in the case of the employment of an achromatic telescope, the light does, to a certain extent, suffer decomposition, and its recomposition is not altogether perfect. Certain it is, that the difference in the manner in which the tints of the features of the planet were brought out was most strikingly evident in the case of the view

furnished by the reflector, as compared with that yielded by the achromatic, however perfectly the latter performed its duty in the important quality of fine definition.

In conclusion, I could not but feel impressed, while beholding this fine view of the planet Mars, that I was looking upon a world! presenting in its remarkable features many close analogies to that of our own, in which respect I am led to consider Mars to be a closer type of the earth, both in its aspect and conditions, than any of the other planets of the solar system.

XXIV.—*On the Wave of High Water; with Hints towards a New Theory of the Tides.* By Thomas Carrick.

Read before the Physical and Mathematical Section, April 30th, 1863.

Professor Airy, in his able treatise on tides and waves, after reviewing in detail the existing causal theories of the tides, passes judgment upon them substantially in the following terms:—

Of the equilibrium-theory of Newton, whilst admitting its great usefulness in some respects, he says that it is one of the most contemptible theories that was ever applied to explain a collection of important physical facts. It is entirely false in principle, and entirely inapplicable in its results.

Of Laplace's theory he says that, although based upon sounder principles than that of Newton, it fails totally in application, from the impossibility of introducing in it the consideration of the boundaries of the sea, and it gives no assistance in explaining the peculiarities of river or channel tides.

And of the theory in which tidal waves are supposed to

run in the manner of ordinary waves in canals, he says that its great and important defect is, that the water is not distributed over the surface of the globe in canals of uniform breadth, or in any form very nearly resembling them. In this regard its fundamental suppositions are probably as much, or nearly as much, in error as Laplace's theory; but it masters the peculiarities of river tides, which no other theory has touched upon.

This ample confession of the imperfections of existing causal theories must plead our excuse for offering a few hints towards a new theory of the tides. These hints are, however, merely incidental to the subject of this paper, which is practically limited to the consideration of the law of direction of the progressive motion of the wave of high water.

Our present knowledge of the progress of tidal waves is derived from observations of the time of high water on ocean coasts—the direction in which the hours increase along a given coast being held to indicate the direction of the progressive motion of the wave. The theories of Newton and Laplace would lead to the conclusion that tidal waves should tend to follow the direction of the moon's motion from east to west; but observation has shown that the real progression is nearly everywhere at right angles to that direction, the hours increasing from north to south, or south to north, indifferently, on the shores of the principal land-areas of the globe. This anomalous progression has, in fact, rendered those theories of little or no value when applied to the consideration of the probable direction of the progressive motion of tidal waves.

In 1833 Dr. Whewell published his first map of co-tidal lines. His method of grouping the facts of tidal hours is based upon the supposition that the observed succession of those hours on ocean coasts was due to the progression of free waves of translation, whose origin was to be traced to

the action of the sun and moon on the principal ocean-areas of the globe.

In 1848, however, he virtually abandoned this hypothesis; for, in his Bakerian Lecture on the tides of the Pacific, he not only expressed grave doubts whether such a supposition rightly represents the mode in which ocean-surfaces obey the action of the sun and moon, but he even ventured upon a new hypothesis, in which the phenomena were attempted to be explained on the supposition of stationary undulations corresponding in period with the period of the moon's apparent revolution.

On this supposition an ocean would be divided into two equal portions, by a middle line running from north to south, forming an axis of no tide, the undulations giving simultaneous high water on the eastern shores and simultaneous low water on the western shores at the same time —the northerly and southerly shores being occupied by revolving waves, giving progressive hours along those coasts only.

This method of viewing tidal phenomena was doubtless partly suggested by the consideration that, in direct opposition to the requirements of existing theories, the height of tides at mid-ocean islands is everywhere found to be comparatively small.

An hypothesis not very dissimilar to Dr. Whewell's had previously been broached by Captain (now Admiral) Fitzroy, who referred the tides to oscillations or libratory motions of the surface of the ocean from west to east and east to west. But, independently of the difficulty of tracing such motions to the action of the sun and moon, both these kindred hypotheses lack the needful support of facts; for whereas they imply the existence of simultaneous high or low water on the easterly and westerly shores of all continents, the east coast of Africa and part of the westerly coast of the same continent alone show any decided approxi-

mation to this condition, whilst on all other open ocean coasts the observed facts are mostly the reverse of what the theories require to sustain them.

It cannot, therefore, excite much surprise that neither of these hypotheses has obtained support, or even claimed much notice; so that, in spite of great anomalies and imperfections, the theory of free ocean waves of translation, as illustrated by maps of co-tidal lines, still holds its ground. But whilst little or no advance has been made towards a sound causal theory of the tides, immense progress has of recent years been made in formulating observations of the details of the varying phases of tidal motion. This progress, however, touches only the minor features of the phenomena. By no existing causal theory can the direction of the progressive motion of the wave of high water along any given coast be predicted where unknown, or accounted for when known, although, when suitable observations have been made at any port, the march of the fluctuations of the wave, both in time and height, which in many parts of the globe obey definite laws depending upon the changing position of the sun and moon, can often be predicted with an approach to minute accuracy.

In all the theories to which reference has just been made, it is to be observed that the disturbing action of the sun and moon is supposed to centre on *ocean-areas*, the *land-areas* of the globe only coming into question so far as the shores of these areas form the bounding lines of the water-surfaces: we, on the contrary, starting from a new hypothesis on the relations of terrestrial matter to cosmical force, have arrived at the conclusion that the tidal motions of ocean-surfaces are caused by a differential action of force centring on all land-areas, and reacting indirectly on the margins of all ocean-areas.

Irrespective of ideas of cause, this apparently arbitrary change of stand-point is found to lead to an empirical law

of the progression of the wave of high water, which comprehends in harmonious relation a greater range of facts than any hypothesis hitherto propounded. It may therefore not be altogether out of place to illustrate this method of grouping the facts of tidal phenomena by some brief allusion to the cosmical speculations which have led to this mode of procedure; and although we decline to indorse the received "nebular hypothesis" as a genetic theory, we shall nevertheless, for the moment, avail ourselves of the ideas and phraseology of that hypothesis, as offering the simplest mode of setting forth with becoming brevity, in this incidental portion of the subject, the manner in which, in our view, terrestrial conditions of matter and force are related to space and to bodies in space.

Assuming the existence of a diffused nebula composed of ultimate atoms of matter, each having a normal rotation on a fixed axis in a uniform direction, and with simple forces of attraction and repulsion, arising thereout in virtue of laws analogous to those by which the poles of magnets, under given conditions, alternately attract or repel each other, then any disturbance of the equilibrium of forces in this nebula might lead either to condensation on the one hand or to greater rarefaction on the other.

Should condensation result, that condensation could hardly be supposed to take the form of a graduated state, passing by insensible degrees from the extreme of solid condensation at the centre, to the extreme of nebulous rarefaction in space. For, taking into account the assumed polarity of the ultimate atoms of matter (an assumption indispensable as a basis for differentiation in any nebular theory from which all existing conditions, relations, and motions of terrestrial matter are to be derived), it is reasonable to assume that the nebulous matter in condensing upon a centre, from causes arising out of diverse molecular groupings of these atoms and their poles, would take

up three successive states, constituting the normal types of the solid, liquid, and gaseous states of terrestrial bodies, the solid matter forming a spherical nucleus everywhere covered with a concentric layer of fluid, and this overlaid with a gaseous envelope. A normal sphere, thus formed by the aggregation of rotating atoms in three distinct states, would acquire a motion of rotation of the mass on a determinate axis by virtue of the retardation of atomic rotation consequent upon molecular aggregation, and would also derive a progressive orbit-motion from conditions arising thereout, the direction of the rotation and orbit-motion of the sphere being determined by the original direction of atomic rotation in the diffused nebula. Our concern, however, at present is not with the rotation and orbit-motion of the mass, but with the local constitution of the condensed normal sphere so acquiring those motions. In virtue of the assumed laws of condensation, the three states of matter so aggregated and superimposed in one sphere would be in stable equilibrium at the respective surfaces of normal contact—the solid with the liquid, the liquid with the gaseous, and the gaseous with the uncondensed nebulous matter of space.

The organic differences between the constitution of these successive layers would have intimate analogy with the like differences which now arise when heat, on passing into a solid body, converts it first into a liquid and then into a gas—the heat or force being mainly absorbed, or becoming latent, in effecting structural molecular changes. In like fashion, but in the inverse direction, a portion of the repulsive force predominating in the primal nebula would pass into the latent state on the condensation of a portion of the nebulous matter into the gaseous form; a further portion would become latent in passing by another abrupt step from the gaseous to the fluid form, and yet another portion by a further change to the solid state.

These three states of matter, thus constituted, would necessarily be in stable equilibrium at the respective surfaces of normal contact—the fluid matter having no properties tending to disturb the equilibrium of the molecular structure of the solid matter, on the one hand, or of the gaseous matter on the other, and the gaseous matter bearing like inoffensive relation to the fluid matter, on the one hand, and to the surrounding nebulous matter of space on the other.

The force exerted upon such a sphere of condensed nebulous matter by another of like origin would, therefore, in virtue of the laws of condensation, act, firstly, through and by the intermediation of the atoms of the diffused nebulous matter of space, thence through and by means of the molecules of gaseous matter, and thence through and by means of the molecules of fluid matter to the solid nucleus beneath—each of these varying states of matter thus forming an indispensable link in the unbroken chain by and through which one cosmical body is related to another and to space.

It must needs be admitted that this mode of viewing the action of the force of gravitation differs widely from that indorsed by modern writers on physical astronomy, in whose works space is treated as a vacuum, so far as ponderable matter is concerned. Such was not the faith of the great founder of the laws of gravitation; for, in his third letter to Bentley, Newton explicitly states that "the idea of one body acting upon another at a distance through a vacuum, without the mediation of anything else, by and through which their action and force may be conveyed to one another, is to him so great an absurdity that he believes no man, who has in philosophical matters a competent faculty of thinking, can ever fall into."

In the laws of gravitation, the motions of the heavenly bodies are proposed as a mechanical problem; and it is

not essential to the solution of that problem that either the exact nature of the causal force, or the mode of its transmission, should be determined. In the science of mechanics, a single force acting along a given line may be replaced by any two or more forces acting in different directions, whose resultant on that line is equivalent to the single force. The introduction of the idea of rotation into the constitution of the ultimate atoms of matter, and the consequent polar character of the action of aggregated rotating spheres upon each other, would therefore merely result, so far as physical astronomy is concerned, in replacing an assumed simple force of attraction by combined repulsive and attractive forces, whose resultant on the given line would be the exact equivalent of the more simple force.

But whilst the precise nature of the causal forces may be comparatively unimportant in problems of pure cosmical motion, the hypothesis of a simple attractive force (an hypothesis which Newton declined to indorse, though adopting its phraseology in his writings) has long been an insuperable barrier in the way of correlating the force of gravitation with the other natural powers.

In an age when the doctrine of correlation is making rapid advances in the estimation of philosophers, this anomaly cannot much longer be allowed to cast reproach on our fundamental ideas of force, and some such basis for differentiating cosmical and terrestrial conditions of matter and force as that which is offered by the hypothesis of the polarity of all matter is obviously one of the first and most important steps towards demonstrating the unity of the physical sciences. Recurring, therefore, to the mode of the transmission of cosmical force, there is nothing inconsistent with the Newtonian theory of gravitation in the suggestion already made, that when, in condensing upon a centre, part of the diffused matter from which the solar

system is supposed by the "nebular theory" to have had its origin takes up the successive forms of solid, fluid, and gas, the relations of atomic attraction and repulsion which subsisted in the primal nebula before its condensation ought equally, under modified conditions, to subsist between the solid nucleus and the remaining uncondensed nebulous matter of space,—the intervening layers of fluid and gaseous matter forming essential links in the extended chain by and through which one centre of cosmical condensation is related to another and to all space.

This mode of viewing the transmission of the force of gravitation leads of necessity to the important conclusion, that, so soon as portions of the solid nucleus of such a condensed sphere emerge above the surface of the fluid covering into abnormal contact with the gaseous envelope, a differential action of enormous magnitude centering upon these upheaved land-areas would be at once originated, the first measure of which would be the cosmical value of the latent forces by which the fluid state of matter was first constituted an essential intermediate link between the solid and gaseous states; or, in other words, the differential force would be equivalent to the absolute value of the tension of the intervening fluid state of matter over the entire areas of upheaval. Upheaved land-areas would thus become centres of disturbed equilibrium of force.

It has long been our belief that the three leading states of existing terrestrial matter have each a relative cosmical value, such as might have resulted from their thus originally forming successive differentiations of one uniform atomic constitution of all matter.

In support whereof, it may briefly be urged that not only is this matter obviously distributed in the three analogous states of earth, water, and air, but each of the simpler forms of inorganic matter can also, under given

conditions of heat, successively assume the solid, liquid, or gaseous state without undergoing chemical change.

This universal threefold relation of terrestrial matter points strongly towards the simple and natural hypothesis that the causal laws which now regulate these interchanges of state are the reflex of fundamental laws underlying the entire constitution of matter in the solar system.

Passing over the possible relation of the first land-upheaval to the changes recorded in geology, through successive disintegrations and reintegrations of the three normal states of matter, and the consequent formation of heterogeneous solids, liquids, and gases, the differential force arising from that upheaval would be the initiating cause in the phenomenon of the evaporation of fluid matter whereby water is lifted up from ocean-surfaces in a comminuted state to infiltrate the lower regions of the atmosphere, thus forming an attenuated vapour-ocean by means of which land-areas are in some small degree compensated for the entire denudation of their normal water covering. This vapour-ocean constitutes an intermediate state of matter—a sort of compromise between the fluid and gaseous states determined by reactions arising out of abnormal contact. It therefore necessarily stands in relations of unstable equilibrium towards other states at all surfaces of contact.

By interactions arising thereout, the simple static conditions of force existing prior to land-upheaval are now partly replaced by more complex phases of force; and thus light, heat, electricity, and magnetism, which are expressions of these complex phases, have their root in local reactions between unstable states of terrestrial matter at surfaces of abnormal contact, these reactions attaining their maximum value at the surface of the earth, where heterogeneous forms of matter in contact, but not in stable equilibrium, are together brought under the tension

of cosmical force, and answer unequally and differently to the strain.

The voltaic battery—in which, by surface-reactions of dissimilar solids and liquids in presence of atmospheric tension, heat, light, electricity, and magnetism are evoked—thus becomes a faithful type of the larger reactions of terrestrial matter when under the tension of cosmical force.

The abnormal phases of force thus indirectly resulting from land-upheaval have become in turn the starting-point of fresh differentiations tending towards another covering of the denuded land, in which the pre-existing states of matter occur in complex combination.

Under the moulding hand of the Great Architect of the Universe, this tendency has had its fruition in the infinitely varied forms of vegetation which constitute the brilliant clothing of upheaved land-areas.

In short, the ceaseless molecular changes and local motions of terrestrial matter would, on this hypothesis, be mainly referred to the differential force arising out of land-upheaval.

Sufficient has now been said to indicate the general character of the cosmical speculations which have led to the grouping of tidal phenomena in relation to land-areas as causal centres.

If the upheaval of land above water originated a differential action of force in manner indicated, then it is obvious that no subsequent changes short of entire re-submersion could altogether neutralize the action of this force; part of the present residual of this force would therefore naturally give rise to a perturbative action centring on land-areas, and attaining a maximum value on the shores of these areas.

For if the normal mode of transmission of cosmical force is through the medium of successive layers of solid

liquid, gas, and the diffused matter of space, each layer when superimposed in the above order, and not otherwise, being in stable equilibrium at the surface of contact with that which bounds it on either side, then the upheaval of land-areas, and consequent denudation of the fluid covering, would render the lines of force directed towards such land-areas less effective for gravitative action than those directed towards ocean-areas; because, in the former, part of the force would be expended in producing molecular and other changes at the surfaces of abnormal contact, and also because the attachment, so to speak, of the lines of force at these surfaces of abnormal contact would constitute an imperfect grip or gravitative hold of one surface on the other, and any deficiency of whatever kind in the effective value of the lines of cosmical tension directed to land-areas would have to be compensated by an added strain in those directed to ocean-areas.

In other words, a residual portion of the differential force would be expended in a direct pull or strain upon the waters nearest the shores of land-areas, tending to draw these waters upwards and towards the land as the centre of perturbative action, and would thus give rise to the wave of high water.

By discussing from this point of view the hours of high water at full and change for the principal places of the globe, as given in the Admiralty Tide-Tables for 1863 (the data being first reduced to Greenwich mean time), we have arrived at the following law of the progression of the wave of high water.

In all land-areas in the northern hemisphere, the wave of high water tends to revolve round the coast in the direction of the hands of a watch, and in like areas in the southern hemisphere against the hands of a watch.

Theoretically, this law should hold good in proportion as land-areas approximate to the circular form, with wide

uninterrupted ocean-spaces all round. In a perfectly circular area of this kind, the differential action would have points of maximum and minimum effect on opposite shores at every instant; these together forming a nodal line, both ends of which would move simultaneously round the coast as the moon moved across the heavens, the wave of high water being everywhere the instantaneous expression of the differential force at its nodal point of maximum action.

On the accompanying maps of the world and of the British Islands (see Plates Nos. XI. & XII.), the land-areas which approach nearest to the prescribed condition are enclosed within one or more circles intersecting the salient parts of the coast.

Taking the northern hemisphere first, the circle round the North American continent necessarily excludes the southern part from Yucatan to the Isthmus of Panama, as well as the Russian territory in the north-west, and the promontory of Greenland on the north-east.

The hours of high water at places upon or adjacent to this circular line, proceeding round the coast in the direction of the motion of the hands of a watch, as required by the law of the northern hemisphere, are as under, the progressive increase of the hour being evidence of conformity with the law.

	h.	m.
North end of Davis Straits . . .	10	2
Cape Race, Newfoundland . . .	10	36
Jedore, Nova Scotia	11	57
Little Egg Harbour	12	7
Ocacroke Inlet	12	8
St. Helena Sound	12	30
Doby Inlet	12	58
St. Simon's Isle.	1	9
Fernandina, Florida	1	19
St. Augustine, Florida	1	47

	h.	m.
Cape Florida	1	54
Key West, Gulf of Mexico	2	37
Cape Catoche, Yucatan	3	18
San Blas, Mexico	4	41
Mazatlan	4	46
San Diego Bay, California	5	27
Monterey	6	29
Port Bodega, near San Francisco	7	28
Port Orford	7	44
Oregon, or Columbia River	8	31
Sitka, Russian America	9	34

The ice-bound northern frontier presents unconformable results; the water-spaces, when not frozen, having much of the character of inland seas.

The continent of Europe, grouped as it is with Asia and Africa, cannot be enclosed within a circular line; and in cases of this kind it is found that where any distinct traces of progression appear, the coast-line is usually divided into two or more segments, each having an independent wave.

Starting from Cadiz, and proceeding to the North Cape, the hours are as under:—

	h.	m.
Cadiz	2	10
Belem, Lisbon	3	7
Cape Finisterre	3	37
Ushant	3	52
Abervrach	4	32
Tregnier	5	45
Alderney	6	55
Cherbourg	7	55
Havre	9	51
Dieppe	11	2
Calais	11	42
Dunkirk	11	58

	h.	m.
Ostend	0	13
Flushing	1	6
Browershaven	2	1
Brielle	2	43
Rotterdam	3	27
Texel	6	12
West Terschelling	8	19
Norderney	10	1
The Elbe (entrance)	11	24
Tonning	1	25
Husum	2	00
Aggerminde	3	51
Skagen, or the Skaw	5	14
Romsdal Isles, Norway	10	20
Lofoten Isles, Norway	11	4
Hammerfest, Norway	11	36

There is here a striking contrast between the velocity of the wave along coasts open to the Atlantic Ocean, as compared with the inner coast-lines flanked by the British Islands. Indeed it would not be inadmissible to draw outside those islands a curve grouping all the open Atlantic shores, and then the hours will be as under :—

	h.	m.
Cadiz, Spain	2	10
Belem, Lisbon	3	7
Cape Finisterre, Spain	3	37
Ushant, France	3	52
Bantry Bay, Ireland	4	20
Donegal Harbour, Ireland	5	50
Cape Wrath, Scotland	7	50
Balta, Shetland Isles	9	48
Romsdal Isles, Norway	10	20
Hammerfest, Norway	11	36

In the British Islands the conditions become very complex, because the essential requirement of wide uninterrupted ocean-spaces exists only to a very partial extent. Where narrow seas, like the Irish Sea and English Channel, separate two or more land-areas, the normal direction of the wave of each area is opposite on the adjacent shores, and hence two waves of equal force might neutralize each other and tend to destroy all progressive motion.

It is impossible to include the whole of England and Scotland in one circle approximating the coast-line; the north of Scotland is therefore severed at the Frith of Forth, England being treated as a separate area.

Taking Scotland first, and passing round in the required direction, the hours are—

	h.	m.
Duncansby Ness	10	26
Wick	11	34
Fraserburgh	12	48
Aberdeen	1	8
Montrose	1	35
Tay River Bar	2	17
Leith	2	30
Crinan (on the west coast)	5	11
Iona Sound	5	37
Loch Moidart	6	8
Loch Torridon	6	43
Loch Laxford	7	4
Cape Wrath	7	50
Loch Eribol	8	1
Thurso	8	42
Swona	9	47
Duncansby Ness	10	26

In Ireland, from causes already alluded to, the westerly and northerly coasts alone are conformable to the law. The hours are—

	h.	m.
Blackball Harbour, Bantry Bay .	4	20
Valentia Harbour	4	23
Fenit, Tralee Bay	4	43
Kilbaha Bay, River Shannon . .	4	56
Clifden Bay	5	10
Broadhaven Harbour	5	39
Donegal Harbour	5	50
Sheephaven	6	4
Port Rush	6	35
Ballycastle Bay	6	50

Here the progress of the wave is greatly retarded, 4 hours and 5 minutes being occupied in reaching Red Bay, which is only 17 miles distant, whence, southward, as far as Wicklow high water is nearly simultaneous, the time averaging about $11^h\ 20^m$.

The south-easterly and southerly coast presents the following unconformable progression :—

	h.	m.
Wicklow	10	53
Arklow	9	10
Kilmichael Point	8	55
Courtown Bay	no tide.	
Wexford	7	46
Saltees	6	6
Youghall	5	45
Kinsale	5	17
Castletownsend	4	58
Cape Clear	4	38
Blackball Harbour, Bantry Bay .	4	20

England presents very complex conditions. The south and east are affected by the combined areas of Europe, Asia, and Africa, the west by Ireland, and the north by Scotland, the continuity of the ocean-boundary in the

north being altogether interrupted. And yet, notwithstanding these adverse influences, marked indications of the law are found to exist.

But, instead of a single wave progressing round the coast within twelve hours, two simultaneous waves move over nearly equal spaces of opposite sides of the island in nearly the same time, with kindred irregularities where the orderly progression is interrupted.

If, therefore, instead of a nodal line with high water at one end and low water at the other, such a line be conceived with high water at both ends, then drawing this line, in the first instance, between Bridlington Bay and Penzance, the wave at the north-eastern end of this line, moving in conformity with the law, will give the following hours:—

	h.	m.
Bridlington Bay	4	40
Spurn Point	5	26
Cromer	6	55
Yarmouth Roads	9	8
Aldborough	10	38
Margate	11	34

At Margate, the orderly progression ceases; and thence to Christchurch, near the Bill of Portland, high water is nearly simultaneous, the time averaging about 11^h 25^m.

There is no trace of this wave westward of Christchurch (except at Poole); but an unconformable wave from the Land's End reaches Christchurch at 9^h 7^m, giving double tides thence to the head of Southampton Water. Westward of Christchurch the hours are as under:—

	h.	m.
Christchurch (wave from Margate) . . .	11	37
Christchurch (wave from Land's End) .	9	7

	h.	m.
Portland Breakwater	7	11
Lyme Regis	6	33
Torbay	6	14
Plymouth	5	54
Falmouth	5	17
Penzance	4	52

Taking now the south-western end of the same nodal line, and moving in the same direction as before, the hours are—

	h.	m.
Penzance	4	52
St. Ives	5	6
Lundy Isle	5	34
St. Ann Lighthouse, Milford Haven	6	17
Cardigan	7	19
Aberystwith	7	47
Bardsey Island	7	59
Caernarvon	9	50
Holyhead	10	29
Amlwch (north coast of Anglesey)	10	47

Thence high water becomes nearly simultaneous over a large extent of coast, the time averaging $11^h\ 20^m$ over all the open coast of Lancashire and Cumberland.

At both ends, therefore, of the nodal line there is an equal progression of the wave over about 90° of arc in about 6 hours of time, then a nearly simultaneous tide over about 45° of arc (at $11^h\ 20^m$ and $11^h\ 25^m$ respectively) at the opposite ends of the line, and then unconformable hours in the south, where the progression of the nodal line in the north is interrupted by the mainland of England.

It is not difficult to point to the probable causes of the abnormal progression of the waves of the south coasts of

England and Ireland. Attention has already been directed to the fact that the wave of open Atlantic coasts moves with great velocity, as compared with those of the inner coast-lines of Europe.

The wave which proceeds from Ushant at $3^h\ 52^m$ along the west of Ireland reaches Hammerfest, near the North Cape, at $11^h\ 35^m$, whilst the derivative wave, which passes from Ushant up the Channel, only reaches Calais at $11^h\ 42^m$, 7 minutes later than the arrival of the kindred wave at the extreme north-west of Europe. This retardation in velocity is accompanied by a great increase in the magnitude of the wave; for whereas on the west coast of France the average height of spring tides is under 16 feet, at Granville and Cancale, on the coast of Normandy, the tides rise to 37 feet. A wave of this magnitude will obviously dominate the wave of the south coast of England; and hence it results that from the Land's End to near Christchurch the normal wave altogether disappears, and is replaced by an abnormal wave, 16 feet in height, at the Land's End, which merges into the normal wave near Christchurch (opposite Granville and Cancale), at which place it has already dwindled down to 5 feet in height.

In like manner the normal wave of the south of Ireland altogether disappears, partly from the action of the wave of the adjacent coast of England, which passes up the Irish Channel with like retarded velocity and increasing magnitude, and partly also from the combined action of the adjacent continents of Europe, Asia, and Africa. Hence an abnormal wave moves along the south coast of Ireland with a maximum height of about 13 feet, but gradually dwindling down until at Courtown Bay it is absolutely extinguished, whence northward, as from Christchurch eastward, high water is nearly simultaneous over a considerable extent of coast.

The small condensed group of the Færoe Isles, situate in

mid-ocean, presents an instance of conformity with the law; for we have

	h.	m.
Fugloe Fiord . . .	11	40
Svinoe Fiord . . .	12	25
Naalsoe Fiord . . .	4	27
Skaapen Fiord (Hestoe)	5	57
Suderoe Fiord . .	6	28
Mygenæs Fiord . .	9	30
Fugloe Fiord . . .	11	40

The continent of Africa extends into both hemispheres, and is so grouped in relation to Europe and Asia, that a revolving wave is not to be looked for; but nevertheless the only segment of coast which is bounded by open ocean conforms to the law, as under:—

	h.	m.
Fernando Po . . .	3	25
Niger River . . .	3	45
Cape Coast Castle .	4	35
Cape Palmas . . .	5	1
Gallinas River . .	7	31
Sierra Leone . . .	8	47
Sal, Cape Verd Isles	9	17
Cape Blanco . . .	12	54
Al Araisch . . .	1	55
Tangier	2	5
Jerba, near Tunis .	2	26

The eastern coast of Asia is much masked by groups of large islands, which give to the intervening water-spaces much of the character of inland seas, in which the waves of opposite shores tend to neutralize each other. No systematic progression, therefore, appears at first sight, but rather a tendency to a simultaneous high water over successive tracts of coast. Nevertheless, if the mean of all

the tidal hours in each successive tract of coast be taken, there is, in conformity with the law, a progressive though somewhat irregular increase in the mean hour, amounting in the whole to between 4 and 5 hours from the Sea of Japan to the entrance of the Gulf of Siam. On the southern coast of Asia there is, in the Bay of Bengal, an approximation towards a simultaneous high water; but the tides are very irregular in the Arabian Sea. In the eastern archipelago the facts are not sufficiently numerous to reveal any traces of systematic progression.

Passing to continents in the southern hemisphere, we shall proceed round each coast, as required by the law, in the direction opposite to the motion of the hands of a watch, the progressive increase of the hour being, as before, evidence of conformity with the law. South America, being nearly surrounded by open ocean, at first sight appears closely to approach the most favourable conditions; but it is impossible to include the whole of the continent within one circle approximating to the coast-line, and it therefore becomes necessary to sever the northern part at the estuary of the La Plata River, and to treat the southern prolongation of this continent as an independent area. Neither of these areas has uninterrupted ocean-boundaries.

The hours of the northern area are as under:—

	h.	m.
Buenos Ayres, La Plata River .	3	53
Isle of St. Sebastian	5	00
Cape Frio	5	28
Victoria, Espiritu Santo Bay .	5	41
Bahia	6	4
Pernambuco	7	5
Cayenne	7	14
Berbice	8	20
Demerara River	8	38

Omitting the northern coast, on which the tides are small, irregular, and not well determined (this part of the coast being virtually the shore of an inland sea), the hours on the western coast are as under:—

	h.	m.
Panama	8	41
Payta	8	45
Malabrigo	10	18
Callao	10	56
Quilca River	12	50
Copiapa	1	14
Coquimbo Bay	1	54
Valparaiso	2	18
Buenos Ayres, La Plata River	3	53

Part of this circle lies north of the equator, and here, as in most coast-lines, numerous unconformable hours are interspersed; but it is not a little curious to note, that in this instance the unconformable hours, both on the eastern and western coasts, may be mainly resolved into counter waves, conforming to the law of the northern hemisphere.

The narrow southern prolongation of this continent cannot be included in a circle following the coast-line in a satisfactory manner; but successive tracts of the coast are traversed by conformable waves as under:—

	h.	m.
Valparaiso	2	18
Maule River	2	49
Arauco Bay	3	7
Valdivia	3	28
Huafo Island	4	59
Anna Pink Bay	5	45
Cape Pillar	6	0
Cape Horn	9	9

	h.	m.
Cape Virgin	1	3
Port Gallegos	1	29
Santo Cruz River	2	3
Port San Julian	3	15
Port Desire	4	33
Port Melo	8	3
Port St. Elena	8	22
Nuevo Gulf	11	18
Port St. Joseph	2	17
Port San Antonio	2	59
Rio Negro	3	10
San Blas	6	9
Union Bay	7	19
Colorado River	8	9
Port Belgrano	10	8
Buenos Ayres, La Plata River	3	53

South Africa, in shape, approximates to an isosceles triangle, with its base on equatorial Africa, and on that base is necessarily influenced by the superior mass of North Africa and of the adjacent continent of Asia. The result is, that on the western coast the law of the northern hemisphere extends to some distance south of the equator, whilst on the east coast the tidal hour is nearly simultaneous. There is, nevertheless, a progression of hours on the south coast, as under:—

	h.	m.
St. Helena Bay	1	18
Table Bay	1	28
Cape Aguillas	1	30
Mossel Bay	1	46
Algoa Bay	2	17
Port Natal	2	26
Shefeen Isles, Delagoa Bay .	2	30

In Australia, where any progression of the tidal wave can clearly be traced, the direction is in conformity with the law of the southern hemisphere. On the east coast we have

	h.	m.
Jervis Bay	8	17
Shoal Bay	10	16
Wide Bay	10	47
Port Bowen	11	32
Flinders Group	11	39
Cape Sidmouth	11	46
Cape York	1	46

The north coast of Australia appears to be largely affected by the adjacent archipelago; and along the north, west, and south coasts, detached places have an almost simultaneous hour, overlaid with many irregular hours. On these coasts there is much reason to doubt the accuracy of the data of the Admiralty Tide-Tables, which are taken in great measure from observations of early Australian discoverers. On the west coast, where English settlements exist, the observed tidal hours would interpolate well with those of the east coast, on the theory of a wave revolving round the continent in 12 hours

The middle island of New Zealand, situate in open ocean, offers a favourable instance of conformity with the law; for we have

	h.	m.
Cape Farewell	9	49
Milford Sound	10	4
Dusky Bay	12	11
Ruapuke Isles, Foveaux Straits	1	48
Otago Harbour	3	28
Cape Campbell	6	22
Queen Charlotte Sound	9	12
Cape Farewell	9	49

In the northern island the progression is also in the required direction, but is much overlaid with irregular hours, arising out of the reactions of the middle island.

It thus appears that, whilst no land-area exists in which the conditions essential to a perfect revolving wave attain to more than a very moderate degree of fulfilment, and although every existing area is more or less subject to the perturbing effects of adjacent areas, yet nevertheless, whenever any systematic progression of the hour of high water can be distinctly traced, the wave of high water in the northern hemisphere everywhere tends to revolve round the coasts of land-areas in the direction of the motion of the hands of a watch, and in the southern hemisphere against the direction of the motion of the hands of a watch.

It may not be out of place to note some facts which strikingly confirm the method of reducing continents, &c., to circular areas of apparently arbitrary position and extent.

The circle which encloses the North American continent emerges, on the coast of the Pacific, at the Mexican port of San Blas, and that which surrounds the main portion of South America emerges on the same ocean at Panama. Between these two points there is a slight progressive increase in the tidal hours, which range from $8^h\ 42^m$ at Panama to $9^h\ 46^m$ at Acapulco; and then there is the curious fact, that between the closely adjacent ports of Acapulco and San Blas there is an abrupt break of several hours in time, as under—

	h.	m.
Acapulco	9	46
San Blas	4	41

although, as has already been shown, the progression northwards from San Blas along the west coast of North America is unusually regular, thus showing conclusively

that the apparently arbitrary position assigned to the northern circle has nevertheless a definite relation to the facts which it professes to group.

In like manner the circle which surrounds northern Scotland emerges on the west coast at Crinan at $5^h\ 11^m$; and here again, as in the case of the American continent, whilst the progression northwards along the west coast of Scotland is regular, there is an abrupt break of several hours to the south of Crinan, with a nearly simultaneous tide at all the ports on the south-west and south of Scotland, the average time being about $11^h\ 20^m$.

Where the same circle emerges on the east coast of Scotland, there is no break in time, but a nearly simultaneous hour southwards from the Frith of Forth to Holy Isle.

This tendency to a simultaneous hour of high water on coasts affected by the close proximity of one or more land-areas is a marked feature in tidal phenomena, contrasting strongly with the orderly progression of the wave elsewhere.

And it is also worthy of note, that whereas 12 hours suffice for the revolving wave round the coast of each of the American continents, an equal time is occupied by the wave of the middle island of New Zealand, and by the wave of the group of the Færoe Islands, thus indicating that large and small areas alike are independent centres of action, the velocity of the wave of each area being proportionate to the entire extent of coast to be traversed.

It is well known by those who have handled the data of tidal hours, that, besides the facts which are selected to illustrate systematic progression, every coast is more or less overlaid with irregular tidal hours; and this feature has become more apparent in proportion as correct data have been accumulated.

To a large extent, the difficulty of accounting for these

irregular hours disappears when the facts are treated in relation to land-areas; for whereas no existing land-area is perfectly circular, or has everywhere wide and uninterrupted ocean-boundaries, or is free from the disturbing action of other areas—conditions essential to uniform progression of the wave,—it follows of necessity that anomalies such as these may reasonably be expected.

The nodal points which revolve round irregular areas cannot be supposed to represent the entire differential action. Every point where a departure from the essential conditions exists will necessarily become the centre of a local residual action; the nodal points will merely represent the leading features of each area, so far as that area tends to conform to the required conditions.

Judging from frequent coincidences in time, these irregular tidal hours in many instances appear to be due to the direct local action of the moon when crossing the meridian of the place.

In like manner interruptions in the continuity of the surrounding ocean-spaces will lead to variations in the velocity of the waves; and this interruption, when extensive, should culminate in simultaneous high water.

It is not consistent with the scope of this paper to enter upon other phases of tidal action; but it may be permitted briefly to allude to the circumstance that our mode of viewing the phenomena of the tides would lead to the conclusion that mid-ocean tides should always be small, and that the wave of high water should everywhere roll in nearly parallel with the coast. Observation has proved that the facts are in intimate harmony with these conclusions, although these facts are directly at variance with the requirements of existing theories.

In short, when approached from our stand-point, every part of the phenomena of the tides will receive more or less of elucidation; and where anomalous results appear, it will

be found that, when rightly considered in relation to disturbing causes, even these will tend indirectly to confirm the method of grouping the data of tidal hours in relation to land-areas as causal centres.

XXV. — *On the Number of Days on which Rain falls annually at London, from observations made during the fifty-six years,* 1807 *to* 1862. By G. V. Vernon, Esq., F.R.A.S., M.B.M.S.

Read before the Physical and Mathematical Section, April 30th, 1863.

Being frequently asked by medical men and others on what number of days rain usually falls during the year, I have compiled the Table accompanying this paper.

The observations made at Somerset House by the Royal Society for the years 1797 to 1830 being in many months not trustworthy (see Howard's 'Climate of London,' 2nd edition, vol. i. p. 132), I have adopted Howard's values for the years 1807 to 1831, given in his 'Climate of London,' vol. i. These observations, although not made absolutely in London, may safely be used without any great error being introduced. From 1832 to 1840 I have adopted the values given for Somerset House, printed in the 'Philosophical Transactious.' From 1841 to 1862 the values are those for Greenwich Observatory. Throughout the entire period of 56 years, there is not a single month in which no rain fell.

The years in which rain fell upon the fewest days were 1832 and 1834, the numbers being 86 and 82 days respectively; 1832 was the cholera year. Rain fell upon the greatest number of days in 1848, the number being 223 days. The mean number of days upon which rain fell

annually during this long period is 155·4 days; the monthly means are as follows:—

	Days on which Rain fell.
January	13·4
February	12·4
March	11·9
April	12·7
May	12·7
June	12·0
July	13·0
August	12·9
September	12·7
October	14·4
November	13·6
December	13·6
Year	155·4

The maximum appears to occur in October, the wettest month, and the minimum in March. The quarterly values are,

	Days.
Winter—Dec., Jan., and Feb.	39·4
Spring—March, April, and May	37·3
Summer—June, July, and August	37·9
Autumn—Sept., Oct., and Nov.	40·7

The maximum here occurs in autumn, and the minimum in spring.

Finding the means for periods of 5 years, we have

			Days.
1st period		1807–1812	168·1
2nd	do.	1813–1817	191·8
3rd	do.	1818–1822	173·2
4th	do.	1823–1827	168·2
5th	do.	1828–1832	154·8

				Days.
6th	period	1833–1837	. . .	137·4
7th	do.	1838–1842	. . .	143·0
8th	do.	1843–1847	. . .	117·6
9th	do.	1848–1852	. . .	175·6
10th	do.	1853–1857	. . .	142·8
11th	do.	1858–1862	. . .	154·8

These figures seem to show that the number of days upon which rain falls undergoes some kind of periodicity. From the 2nd to the 8th period there is a continued falling-off in the number of days on which rain fell, with the exception of the slight advance in the 7th period; there is a great rise at the 9th period, followed by a considerable falling-off in the 10th period. From the advance in the 11th period, it would appear as if we were returning to the same conditions as those prevailing during the first five periods.

Days on which Rain fell at London, 1807–1862.

Year.	January.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.	Total Days for Year.
1807.	7	14	7	6	17	6	8	9	7	10	11	6	108
1808.	11	11	4	15	12	9	15	15	18	15	10	16	151
1809.	22	22	9	24	13	11	15	21	23	11	16	24	211
1810.	16	23	15	10	10	4	20	17	6	11	22	19	173
1811.	12	15	7	13	20	11	13	19	11	19	15	19	174
1812.	13	22	22	14	19	16	12	14	6	25	13	16	192
1813.	13	15	9	12	23	14	13	15	13	21	15	16	179
1814.	19	8	18	15	9	23	18	18	14	16	17	24	199
1815.	12	14	20	19	18	13	20	13	11	15	16	17	188
1816.	19	14	16	12	17	11	27	22	14	19	15	20	206
1817.	21	19	15	12	19	12	16	18	7	18	12	18	187
1818.	23	13	25	19	15	12	10	4	22	11	17	9	180
1819.	16	22	14	16	12	15	8	4	12	15	15	16	165
1820.	18	13	11	11	17	16	10	12	14	16	17	16	171
1821.	13	6	24	17	14	11	12	13	19	17	22	25	193
1822.	8	8	14	22	11	7	14	14	9	23	21	6	157
1823.	15	19	14	13	14	10	21	23	8	13	8	18	176
1824.	8	17	18	18	17	15	12	17	18	17	19	21	197
1825.	12	10	10	10	10	12	3	15	10	15	19	18	144
1826.	6	16	11	10	12	4	10	11	15	14	15	14	138
1827.	20	11	20	17	14	15	9	16	14	15	14	21	186

Days on which Rain fell at London (*continued*).

Year.	January.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.	Total Days for Year.
1828.	16	16	13	17	11	14	25	13	13	9	7	13	167
1829.	11	13	7	25	9	15	22	21	19	11	12	11	176
1830.	22	16	8	14	15	19	13	17	21	8	14	13	180
1831.	14	18	15	10	10	13	12	11	15	15	14	18	165
1832.	6	1	4	6	7	12	3	11	5	14	7	10	86
1833.	5	15	6	22	3	17	10	4	5	11	8	18	124
1834.	17	3	7	2	8	9	12	9	6	4	3	2	82
1835.	2	5	3	11	14	11	4	6	14	15	11	5	101
1836.	9	10	19	19	6	12	9	9	17	16	19	12	157
1837.	14	11	6	7	9	10	10	14	10	11	11	10	123
1838.	11	9	9	11	9	20	18	14	10	8	17	11	147
1839.	11	13	12	10	9	12	12	10	21	13	18	13	154
1840.	16	13	4	3	12	11	14	11	12	10	16	2	124
1841.	10	10	13	15	12	10	18	15	15	22	13	18	171
1842.	7	11	13	5	14	6	13	7	16	4	18	5	119
1843.	8	14	4	9	19	12	14	10	3	18	14	3	128
1844.	9	15	9	3	3	7	13	10	7	15	13	6	110
1845.	12	4	5	7	15	11	14	10	11	8	13	10	120
1846.	16	7	14	16	8	5	8	12	4	21	9	8	128
1847.	9	8	4	9	11	10	4	7	12	10	8	10	102
1848.	8	19	22	23	5	22	18	29	14	26	19	18	223
1849.	17	19	11	20	15	7	12	8	15	21	11	18	174
1850.	10	13	5	18	21	8	15	14	13	8	14	16	155
1851.	15	14	21	11	12	12	17	9	14	14	10	6	155
1852.	19	12	5	6	14	23	4	16	13	17	23	19	171
1853.	20	13	13	14	11	13	16	7	12	24	11	8	162
1854.	15	9	6	7	17	12	15	12	9	11	13	16	142
1855.	20	11	12	4	12	9	10	10	6	22	17	11	144
1856.	18	10	6	13	18	7	13	10	17	10	10	13	145
1857.	20	3	10	18	5	9	9	11	13	9	8	6	121
1858.	5	6	8	11	17	5	12	8	10	9	7	14	112
1859.	11	12	10	13	9	7	7	11	17	18	13	17	145
1860.	21	13	18	13	14	23	10	25	17	10	11	17	192
1861.	6	11	21	6	8	15	20	9	15	10	15	10	146
1862.	17	6	22	13	16	16	16	14	18	17	8	16	179
Sums...	751	694	668	716	711	671	728	724	710	805	764	762	8704
Means	13·4	12·4	11·9	12·7	12·7	12·0	13·0	12·9	12·7	14·4	13·6	13·6	155·4

XXVI.—*On the Rain-fall at Oldham during the years* 1836 *to* 1862. By John Heap, Esq.; with Remarks by G. V. Vernon, Esq., F.R.A.S., M.B.M.S.

Read before the Physical and Mathematical Section, April 30th, 1863.

The observations given in this paper were made at Royton, near Oldham. During the period 1836 to 1852, a 12-inch circular gauge was used, with a float, and situated 24 feet above the ground. From 1853 to 1857, the height was only 11 feet above the ground. From 1858 to 1860, the gauge used was a 10-inch, with float, and 11 feet above the ground. In 1861 and 1862 the float was dispensed with, and the amount of rain fallen estimated by weight, the gauge remaining, as before, 11 feet above the ground.

The mean fall for the first 17 years, 24 feet from the ground, was 32·468 inches. The mean fall for 1853–1857 was 30·802, 11 feet from the ground. The mean fall, 1858 to 1862, was 38·069 inches, also 11 feet from the ground. Combining these last two series, we have 34·432 inches for the fall, 11 feet above the ground. The corresponding periods at Manchester give, 1836 to 1852, 36·859 inches; 1853 to 1857, 31·371 inches; 1858 to 1862, 33·757 inches; and 1853 to 1862, 32·564 inches.

During the first 17 years the fall at Oldham appears to be greatly below that at Manchester; but this is owing to the elevation of the gauge. The fall during 1858 to 1862 would seem to approach nearer the normal fall for Oldham, as compared with the average of the same period for Manchester, since, the locality being considerably higher, much more rain might be expected to fall. If the ratio between the rain-falls at Oldham and Manchester during the period just alluded to held good for the entire period 1836 to 1862,

assuming the height of the gauge to be 11 feet above the ground, it would give 39·752 inches for the mean rain-fall at Oldham for 27 years. Although Oldham is fully 200 feet higher than Bolton, the rain-fall at Oldham is greatly below that at Bolton: for example, the rain-fall at these two stations in 1860, 1861, and 1862 was as follows:—

	1860.	1861.	1862.
	inches.	inches.	inches.
Oldham............	44·023	33·084	41·238
Bolton	57·660	44·910	53·430
Difference	13·637	11·826	12·192

or a mean difference of 12·551 inches. The data at Bolton are from Mr. H. H. Watson's returns, 290 feet above the sea, and $3\frac{1}{2}$ feet above the ground. No doubt the greater fall at Bolton is owing to the vicinity of high lands, including the large mass of Rivington Pike. Part of these differences is caused by the gauge at Oldham being placed $7\frac{1}{2}$ feet above that at Bolton, referred to the surface of the earth as datum. The values for the mean rain-fall at Oldham for 27 years, given in Vol. III of the Proceedings of our Society, page 112, are too small, owing to the great height the gauge was placed above the ground during the first seventeen years. Mr. Heap informs me that, from the beginning of the present year, his gauge has been lowered to 4 feet above the ground.

The Tables appended to this paper are, 1st, monthly fall for 27 years at Oldham; 2nd, comparative annual falls at Oldham and Manchester; 3rd, mean monthly rain-fall at Oldham during each period that the same gauge and system of measurement was adopted.

TABLE I.—Rain-fall at Royton, near Oldham, 500 feet above the sea.

	1836.	1837.	1838.	1839.	1840.	1841.	1842.	1843.
January	2·109	2·347	0·231	1·971	2·501	1·641	1·880	3·580
February ...	2·502	2·569	1·159	1·629	1·432	1·200	1·961	0·521
March	2·739	2·053	1·951	1·252	0·320	0·258	3·160	0·890
April	2·341	0·987	2·862	1·538	0·578	1·229	0·978	3·599
May	0·759	2·181	2·938	0·332	4·402	3·641	2·839	2·652
June	4·262	2·058	4·291	1·991	3·310	4·028	2·068	2·000
July	4·271	1·700	2·038	1·702	6·389	6·690	3·664	1·052
August	1·419	3·039	4·704	3·233	3·660	3·470	1·450	3·918
September ...	2·991	1·545	0·710	4·981	5·318	5·269	1·929	0·590
October	3·500	3·890	4·329	2·572	1·732	0·712	2·140	4·958
November ...	5·959	5·268	0·682	2·611	1·310	5·930	3·489	5·670
December ...	3·667	3·402	1·550	1·672	1·559	0·578	2·550	1·700
	36·519	31·043	27·445	25·484	32·521	34·646	28·108	31·130

TABLE I. (*continued*).

	1844.	1845.	1846.	1847.	1848.	1849.	1850.	1851.
January	1·341	1·970	4·000	1·831	3·010	2·610	1·890	1·642
February ...	1·052	1·241	1·082	1·450	3·990	3·252	2·191	3·964
March.........	2·220	2·814	1·240	3·418	3·414	1·621	3·684	2·630
April	1·000	1·887	4·931	2·830	3·138	3·612	2·120	1·034
May	0·000	1·980	1·380	1·281	3·250	2·154	3·842	3·144
June	1·632	3·000	3·618	2·650	3·749	4·180	3·166	2·631
July	0·852	5·974	6·491	4·310	3·003	2·151	4·250	1·180
August	2·120	5·520	2·780	3·618	4·860	3·430	3·990	3·000
September ...	3·012	3·881	1·070	3·004	3·141	2·821	3·822	2·132
October	1·920	4·110	5·238	3·260	3·310	3·191	4·164	2·610
November ...	2·994	2·772	2·562	3·842	3·010	3·690	3·140	2·184
December ...	0·510	6·620	1·860	2·638	2·134	2·610	2·634	1·860
	18·653	41·669	36·252	34·132	40·009	35·622	38·893	28·011

TABLE I.—Rain-fall at Royton, near Oldham, 500 feet above the sea (*continued*).

	1852.	1853.	1854.	1855.	1856.	1857.	1858.
January	2·770	1·752	3·194	0·370	2·554	1·904	3·196
February	1·886	2·442	2·940	0·424	3·236	1·608	2·973
March	1·864	2·200	1·472	1·850	0·033	1·990	1·452
April	1·840	2·112	1·478	0·984	2·454	1·674	2·478
May	2·804	3·440	1·870	1·520	3·154	1·542	2·560
June	3·310	2·692	2·574	1·894	2·144	2·590	3·782
July	2·864	3·702	3·620	4·250	4·200	1·608	2·224
August	5·170	3·462	2·634	3·664	4·708	2·320	1·482
September	2·742	3·194	2·651	0·920	0·704	2·090	4·650
October	3·280	3·755	2·410	5·764	6·362	4·894	3·452
November	2·671	2·771	3·600	1·456	0·450	2·100	1·314
December	2·180	3·084	6·400	0·854	2·484	3·810	3·550
	33·181	34·606	35·083	23·950	32·583	28·130	32·713

TABLE I. (*continued*).

	1859.	1860.	1860.	1861.	1861.	1862.	1862.
			Days.		Days.		Days.
January	2·014	3·631	23	1·220	12	2·622	17
February	2·180	1·112	17	3·144	16	0·736	11
March	3·884	3·754	24	5·244	18	3·073	17
April	3·032	1·682	16	1·880	9	2·079	24
May	0·250	4·614	24	0·724	13	4·636	23
June	2·514	7·062	28	2·800	18	3·332	20
July	3·881	3·334	18	4·062	20	4·332	24
August	6·120	4·270	30	2·940	25	2·545	16
September	6·512	3·400	22	4·482	20	4·064	16
October	3·174	5·362	24	1·750	16	6·360	19
November	2·360	3·250	18	2·460	18	1·737	12
December	3·504	2·552	19	2·378	17	5·091	21
	37·318	44·023	263	33·084	202	41·238	220

Table II.—Comparative Rain-fall at Oldham and Manchester.

Year.	Manchester.	Oldham.	Difference.	
	inches.	inches.	inches.	
1836.	45·351	36·619	− 8·732	
1837.	33·137	31·043	− 2·094	
1838.	31·418	27·445	− 3·973	
1839.	33·349	25·484	− 7·865	
1840.	34·291	32·521	− 1·770	
1841.	41·190	34·646	− 6·544	
1842.	31·555	28·108	− 3·447	
1843.	38·155	31·130	− 7·025	
1844.	26·755	18·655	− 8·100	During this period the gauge at Oldham was 24 feet above the ground; float-gauge 12 inches diameter.
1845.	41·415	41·669	+ 0·254	
1846.	33·350	36·252	+ 2·902	
1847.	43·355	34·132	− 9·223	
1848.	45·230	40·009	− 5·221	
1849.	36·070	35·622	− 0·448	
1850.	34·330	38·893	+ 4·443	
1851.	31·930	28·011	− 3·919	
1852.	45·730	33·181	−12·549	
1853.	32·410	34·606	+ 2·216	
1854.	31·860	35·083	+ 3·223	
1855.	26·425	23·950	− 2·475	Oldham: gauge 11 feet above the ground; float-guage 10 inches diameter.
1856.	34·220	32·583	− 1·637	
1857.	31·940	28·130	− 3·810	
1858.	29·434	32·713	+ 3·279	
1859.	34·495	37·318	+ 2·823	
1860.	36·530	44·023	+ 7·493	Oldham: gauge 11 feet above the ground; 10-inch gauge, but fall of rain determined by weight in 1861 and 1862.
1861.	29·727	33·084	+ 3·357	
1862.	38·598	41·238	+ 2·640	
Means ...	35·268	33·202	− 2·066	

Table III.—Mean Monthly Rain-fall at Oldham.

Month.	1836–1852.	1853–1857.	1858–1862.	1853–1862.
	inches.	inches.	inches.	inches.
January	2·195	1·955	2·537	2·246
February	1·825	2·130	2·029	2·079
March	2·090	1·509	3·481	2·495
April	2·147	1·740	2·230	1·985
May	2·328	2·305	2·577	2·441
June	3·055	2·379	3·898	3·138
July	3·490	3·476	3·567	3·5[illegible]1
August	3·493	3·358	3·471	3·414
September	2·880	1·912	4·622	3·267
October	3·230	4·637	4·020	4·328
November	3·399	2·075	2·222	2·148
December	2·336	3·326	3·415	3·370
Sums	32·468	30·802	38·069	34·432
Corresponding periods at Manchester.........	36·859	31·371	33·757	32·564

XXVII.—*Further Observations on the Carboniferous, Permian, and Triassic Strata of Cumberland and Dumfries.* By the President, E. W. Binney, F.R.S., F.G.S.

Read October 20th, 1863.

Introductory Remarks.

When, in 1848, the Red Sandstones of the neighbourhood of Dumfries first came under my observation, in company with my friend Prof. Harkness, doubts arose in my mind as to the propriety of their being classed with the Trias, their character and organic remains clearly indicating more of a Permian age *. Accordingly, in my first paper published on this subject in the Society's 'Memoirs' † in 1855, allusion is made to these beds, and they are classed as Permian after tracking the Permian beds of Lancashire through the north-western counties of York, Westmoreland, and Cumberland. My attention was chiefly directed to the red marls, magnesian limestone, conglomerate, and soft Red Sandstone strata, these being the common Lancashire types; and where the Red Sandstones of the neighbourhood of Carlisle and St. Bees were incidentally mentioned, they were treated as Upper New Red Sandstone or Trias, as Prof. Sedgwick had described them in his valuable memoirs; but in my 2nd memoir ‡, published in 1857, where the Howrigg, Shawk, and Westward sections are described, I came to this conclusion:—" the

* In the 'Quarterly Journal of the Geological Society' for 1851, p. 162, Sir R. I. Murchison doubted the sandstone of Dumfries being of Triassic age, and preferred to class it with the Permian.

† " On the Permian Beds of the North-west of England," vol. xii. p. 209, of the Society's 'Memoirs.'

‡ " Additional Observations on the Permian Beds of the North-west of England," vol. xiv. p. 101, of the Society's 'Memoirs.'

brick-red sandstones of those places, with their underlying red clays, as well as the breccia at Shawk, I have little doubt will be proved to be Permian. It is true that no fossil organic remains have yet been found in them, with the exception of the tract alluded to in this paper; but if mineralogical characters and geological superposition are to be taken as evidence of their age, they are as good Permian beds as those of West House, Kirby Stephen, and Brough, in England, and Dumfries and other places in the south-west of Scotland, with the latter of which they are most probably connected."

In a paper published by Prof. Harkness, in 1862 *, that geologist adopts, in substance, this view, and agrees with my opinion of the Howrigg, Shawk, and Westward red clays and sandstones being of Permian age, and describes a very beautiful section at Hilton, in Westmoreland, which strongly confirms it. Of course, it was not intended to question the Triassic age of the soft red sandstones of Dalston and Holmhead, near Carlisle, which are covered bv waterstones, red marls, and lias, as stated in my paper on the latter deposit †.

The Shawk sandstones are well seen at Westward Chapel, near Wigton; West Newton, near Aspatria; near Allonby, and to the north of Maryport; and after the Maryport, Workington, and Whitehaven coal-field is passed, they appear again to the south of the coast in the magnificent promontory of St. Bees Head, and continue southward certainly to Netherton, Seascales, Gosforth, Drigg Cross, and probably, as Prof. Sedgwick suggests, into Furness ‡.

* 'Quarterly Journal of the Geological Society' for August 1862, p. 205.

† *Ibid.* for May 1859, p. 549.

‡ Professor Sedgwick "On the New Red Sandstone Series in the Basin of the Eden," vol. iv. new series of the Transactions of the Geological Society of London, p. 389. All geologists who have investigated the geological structure of the counties of Lancaster, Westmoreland, York, and Cumberland must class the venerable Professor as the father of Permian geology in these counties. The more I investigate these districts, the more I find to

On the north of the Solway, the Permian strata on the opposite side of the Vale of Eden are well exposed in the Riddings Junction section, on the Waverley line of railway, about Carwinlay, Moat, and Canobie; and the range of the Moat sandstone, the same age as that of Shawk, by Glenzier Quarry, Cove, near Kirkpatrick, Fleming, above Annan, on to Dumfries, is well marked.

In addition to a description of several Permian sections, two sections will be given which show the occurrence of the upper coal-measures similar to those described by me, some years since, in the valley of the Ayr, near Catrine, thus rendering it extremely probable that such strata extend under the valleys of the Eden and the Esk, their southern outcrop being exposed in the Raw Beck, south of Dalston, and their northern outcrop at Canobie. These carboniferous strata may not be rich in coal; but they contain the limestone of Ardwick, Leebotwood, and Ballochmoyle Braes (formerly termed a freshwater one *), and show a great development of coal-measures, which are useful to be known, if it be only to show the depth that has to be sunk through before the middle and profitable coal-fields of Whitehaven and Canobie can be reached. This portion of the coal-measures, both in Scotland and the north-west of England, has generally been termed Permian, and summarily dismissed as unprofitable "red measures." In my paper on the Ballochmoyle limestone †, it was

admire in the vast amount of original information and truly philosophical deductions which characterize his invaluable memoirs. When I published my first and second papers, I had not carefully read those memoirs, or I should have referred to the learned Professor's notice of the Carboniferous Limestone at Shawk as he saw and correctly described it nearly thirty years before me.

* In this paper it is intended to use the term "Spirorbis Limestone" for freshwater limestone.

† "On some Upper Coal-measures containing a Bed of Limestone at Catrine, in Ayrshire," 'Quarterly Journal of the Geological Society' for November 1862, p. 437.

shown that a great thickness of unprofitable coal-measures had to be traversed before the profitable coal-field at Common could be reached in that district, some 550 yards.

The Canobie section exposes far more coal-measures above the Spirorbis limestone than the one at Ballochmoyle—at least 200 yards; and it shows a passage of Carboniferous into Permian beds, so far as the eye can detect, better than any that has hitherto come under my observation. The strata of these two formations in the bank of the river above the bridge at Canobie, from the lowest bed of breccia into the underlying clays and shales, are most difficult, if not impossible, to separate from the red shales and sandstones seen between that point and the bridge there.

The district about Canobie, Penton, and Longtown has been described at length by Mr. Edmund Gibsone, in an elaborate and well-illustrated memoir, printed in the 'Transactions of the North of England Institute of Mining Engineers'*. In the Penton Linns section, the author describes the mountain-limestone seams of coal; in the Penton Railway section, the millstone-grit series; and in the Canobie coal-field, the middle series; and he shows a fault on the south of the latter coal-field, which throws the coal-measures down, and brings in the Permian strata. All the red measures south of this fault Mr. Gibsone appears to consider Permian, and the fault which brings them in he calls the Great Permian fault. After examining these red measures, I have come to the conclusion that, although a portion of them are Permian strata, as Mr. Gibsone describes them to be, a great part of them are unquestionably upper coal-measures. The profitable Canobie coal-field, like the Common coal-field in Ayrshire, belongs to the middle or

* "A Geological Paper on the Border Districts of Dumfriesshire, Cumberland, and part of Roxburghshire, including the Coal Formation of Canobie, &c.," by Edmund Gibsone, vol. xi. p. 65.

valuable coal-field; but there is also at Canobie a great thickness of upper coal-measures, containing a seam of limestone in all respects like the Ballochmoyle Braes, near Catrine, the Ardwick, and Leebotwood limestones. Consequently the Permian fault should be called by some other name; say, the Great South fault. Practical mining engineers have frequently classed all the red and variegated beds which they find in the upper part of the coal-measures as "red measures" or Permian strata. Now there is, no doubt, often great difficulty in drawing the line of demarcation between the upper coal-measures and the Permian strata; and it is possible that in some sections one may pass into the other, as appears to be the case in the river section above the bridge at Canobie previously alluded to; but in the north-west of England this transition is not generally to be seen. The further we investigate the organic remains of these two formations, probably more genera and species will be found to be common to both than is at present supposed; but in all cases where the remains of *Stigmaria* and *Spirorbis carbonarius* (*Microconchus*) have been found in the strata, I have termed them carboniferous. In the absence of organic remains, which is generally the rule, and not the exception, the Permian character of the strata has been decided by the mechanical character of the deposits and the order of superposition, the beds of breccia and the soft red sandstone generally affording pretty good evidence of the Permian age of the strata over a great extent of country, and varying with the character of the older rocks found *in situ* in the district. If the Permian beds are taken as the Moat sandstone, the red shales with gypsum and magnesian limestone and breccias lying in soft red sandstone at Canobie, their identification is pretty easy; but in continuing them downwards into the upper coal-measures, or in tracing their boundary upwards into the Trias, there is greater difficulty, as good natural sections, showing the passage of

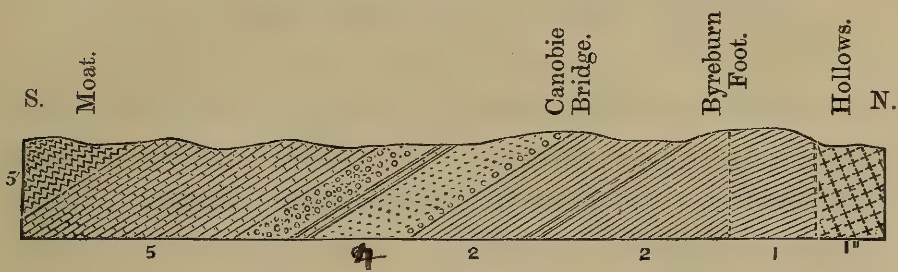
one into the other, are not often met with; but I consider the soft red sandstone of Longtown, West Linton, Rockliffe, and Dalston to be of Triassic age, and covered by the waterstones and red marls of Carlisle, and these, in their turn, to the west overlain by the lias of Quarry Gill and Oughterby.

In the valleys of the Esk and Liddel, and their tributary streams, are some very interesting sections. Raeburn, which falls into the River Lyne, contains in its upper part Permian beds in the form of some soft red sandstones; but I could not find the breccia *in situ*, although large blocks of it occurred in the watercourse. The same remark applies to the Archer Beck Burn; but in the larger valleys of the Esk and the Liddel, as well as in the smaller one of Carlingway Burn, I found beds of breccia *in situ*, and therefore my observations will be confined chiefly to those places.

A short description of the upper red marls and waterstones found at Carlisle, with the underlying red sandstone, which are of Triassic age, will be given. The latter rock will be traced up the valley of the Caldew, by Holmhead and Dalston, to a place below Holm Hill, where a bedded and rippled sandstone makes its appearance. This sandstone cannot be distinguished from that of Shawk, of which probably it is a continuation. Further up the valley, a little above the junction of the Raw Beck with the Caldew, an upper coal-field is seen, with a Spirorbis limestone like that of Canobie, which is succeeded on its rise by soft red sandstones, probably of Permian age.

Prof. Sedgwick, many years since, described the Whitehaven sandstone as Lower New Red Sandstone; and, after several visits to the district, I am inclined to endorse that opinion, as I cannot find any difference between this sandstone, especially the lower part of it, and my Lower Permian of Astley, Bedford, and Moira, near Ashby-de-la-Zouch.

Moat and Canobie Section. Distance 3½ miles.*



In the valley of the Esk, at Longtown, a soft red sandstone, which crumbles on being rubbed between the fingers, is found lying nearly level, with a dip, if any, slightly to the west. This rock underlies the greater part of the country by Netherby and Scotch Dyke, until you reach the dark red sandstone of Moat; but the passage of the former rock into the latter is not well seen. However, in the Moat sandstone there is a finely laminated and small-grained stone, suitable for building-purposes, with ripple-marks and a few desiccation-cracks on its surface. Its colour is generally of a dark red, but in its lower beds it is drab. On the whole, it is so like the sandstone of Shawk and St. Bees on the south, and Glenzier and Cove on the north, that there can be no doubt as to its being, with them, of Permian age. The whole thickness of the stone exposed

* In this and the following sections, illustrating the present memoir, the references will be the following:—

Trias ....
- 7. Upper Red Marls and waterstones.
- 6. Upper New Red Sandstone, Bunter.

Permian ....
- 5'. Shawk or St. Bees Sandstone.
- 5. Red marls, with gypsum and conglomerate or breccia.
- 4. Lower New Red Sandstone. At Canobie, Nos. 4 and 5 both contain beds of breccia.
- 3. Red clays.
- 3'. Whitehaven sandstone and pebble-beds, Lower Permian.

Carboniferous ..
- 2. Upper Coal-measures.
- 1. Middle Coal-measures.
- 1'. Lower Coal-measures.
- 1". Mountain Limestone series.

in the quarry is not more than 30 feet, and it dips to the west at an angle of 8°. It passes downwards into a deposit of red shaly clays, containing thin veins of gypsum, and occasional bands of sandstone, of between 200 and 300 feet in thickness, which underlies the valley up to the turn of the River Esk in Canobie Holm, where a dislocation, in the shape of a small anticlinal axis is seen, near the Round and Long Pools.

This axis shows a singular fine-grained stone of a greenish tint, beds of red sandstone containing hard bands, large nodules, and a breccia of 3 feet in thickness, composed of fragments of red sandstone and limestone in a red clay paste, dipping to the south-west at an angle of 34° on the one side, and on the other side beds of red clays and sandstones, dipping to the north-west at first at a greater angle, but gradually lessening until a bed of breccia, composed of fragments of red sandstone and limestone in a red paste 6 feet in thickness, make their appearance. These are succeeded by a bed of dark red sandstone, mottled with marks of brown and drab colours, 25 yards in thickness, dipping to the north-west at an angle of 12°.

For a short distance the strata are not visible; but in the bank of the river, below Canobie Kirk, they are again seen in the form of a soft sandstone of a bright red colour, containing a bed of breccia composed of small limestone-pebbles in a red paste of sandy clay of 8 inches in thickness. This is succeeded by bright red clays and red sandstones. The dip of the strata here is to the south, at an angle of 10°. Then comes a bed of thick red sandstone, followed by a light-coloured sandstone and red shales, containing some thin beds of magnesian limestone of about a yard in thickness altogether, which dip to the south at an angle of 15°. The following is an analysis of this limestone, for which I am indebted to Mr. M. Binney, of the Bathgate Chemical Works: viz.,

Carbonate of lime	58·00
Carbonate of magnesia	32·46
Iron	1·00
Silica	4·88
Alumina	2·43
Water, loss, &c.	1·23
Specific gravity, 2·73	100·00

From this limestone to the bridge the distance is occupied by a bed of soft red sandstone, with a few clay partings in it of about 90 yards in thickness, which terminates just above the bridge*, and is succeeded by about 2 yards of breccia, composed of carboniferous grit-stones and limestones. The thick sandstone has a dip from 15° at the southern commencement, increasing to 35° at its northern boundary, towards the south. The dip of the underlying breccia was not so well seen, but it appeared to be in the same direction as its overlying sandstone. Underneath this breccia was a bed of red shales, containing the rootlets of *Stigmaria ficoides*. I did not see these red shales actually pass into the breccia, owing to a covering of about 5 yards of fallen bank; but they appear to dip in the same direction, namely, slightly east of south, although at a somewhat less inclination.

With these red shales I consider the coal-measures to commence, the Permian strata to terminate at the lowest bed of breccia. The Permian beds in this section I roughly estimate at the following thicknesses in the descending order:—namely,

	Yards.
The Moat sandstone, as exposed in the quarry, but doubtless much thicker on the dip	10
Red shaly clays, containing bands of gritstone and thin veins of gypsum	75
Soft red sandstones, parted by red clays, and containing a bed of magnesian limestone, four different beds of breccia, one of which forms the base of the series	200

* Prof. Sedgwick ("On the New Red Sandstone Basin of the Eden and the North-western coasts of Cumberland," &c., 'Transactions of the Geological Society of London,' 2nd series, vol. iv. p. 385), in speaking of the north-eastern

No doubt the anticlinal axis previously noticed as seen near the Round and the Long Pools in the Esk, might have repeated some of the beds; but the four beds of breccia appear to me so different in characters, boundary-rocks, and thicknesses, that I came to the above conclusion.

In this section the lower soft red sandstone, instead of being a compact mass, lying under the magnesian limestone and the breccia or conglomerate, as is generally the case in most of the sections lying to the south, is actually divided into several beds by a bed of magnesian limestone and four different beds of breccia.

The red shales lying under the last bed of breccia, containing *Stigmaria* rootlets, are considered by me to be the highest coal-measures ever yet noticed in Great Britain.

Probably the passage of the carboniferous into the overlying Permian beds is more apparent than real, and the bed of breccia doubtless shows a period of disturbance; but in the whole course of my observations, extending over 30 years, I must say that I have never seen anything before which to me appeared so nearly to prove the passage of the one into the other as this section does.

On continuing the section from near the bridge up the river, red and purple shales, with thin beds of gritstone, are seen for 200 yards to a bed of red and purple-coloured sandstone exposed at Knotty Holm *, of about

boundary of the New Red Sandstone, says, "It passes to the east of Brampton, after which it ranges in a sinuous line, very much covered by alluvial detritus, but on the whole nearly due north, till it crosses the Liddel and enters Scotland; then it is deflected nearly to the west, and crosses the Esk just above Canobie Bridge."

* Prof. Harkness, in a paper published on the New Red Sandstone of the southern portion of the Valley of the Nith, in speaking of what he calls "the great Triassic formation," says, "The eastern limit of the New Red Sandstone in Dumfriesshire is in the parish of Canobie, where it is seen in the bed of the River Esk, at Canobie Bridge. Its northern extremity in this parish is met with a little higher up the river, at a place called Knotty Holm, near to which the Canobie coal-field commences." (Quarterly Journal of the Geological Society for November 1850, vol. vi. p. 389.)

75 yards in thickness, which on the whole dips to the S.S.E. at an angle of 18°, although by a small fault seen on the east side of the river there is a steeper dip to the S.S.W.

This sandstone, in its upper portion, presents no remarkable characters, and differs in nothing from an ordinary carboniferous sandstone; but in its lowest part there is a mottled bed of 14 inches in thickness full of peroxide of iron and red clay, containing fossil wood and coal-plants. The species of the latter are not easy of recognition, with the exception of the *Calamites approximatus*, of which I obtained a good specimen and some fragments of *Dadoxylon*. The bed reminded me of a similar one at Penton, described in the next section, of which it is probably a continuation. In some of its characters it resembled the Whitehaven sandstone. Considerable time was spent in searching for white-quartz pebbles in it; but none were found, with the exception of a small one of the size of a bean, which was met with, not in, but only loose on the outside of the rock; so its occurrence there was not of much value.

Proceeding up the river, some red shales, containing *Stigmaria ficoides* and thin gritstones, reaching to about 30 yards in thickness, are seen. These are succeeded by about 20 yards of red and purple-coloured shales and clays, containing several bands of gritstone, two seams of calcareous ironstone, and a bed of limestone 6 inches in thickness. This latter stone has a porcelain-like fracture, and is of mottled, purple, and cream colours. It contains the *Spirorbis carbonarius* and a *Cypris*?, and cannot be distinguished from the Ballochmoyle limestone described by me in the upper coal-measures near Catrine in Ayrshire, and the Ardwick and other limestones found in the same position in England. From this limestone to the highest carboniferous strata, previously described above the bridge,

there must be a thickness of about 200 yards; so here we have to add that distance to the thickness of the upper coal-measures as seen in Ayrshire. After leaving the Spirorbis limestone, and proceeding up the river, the strata are a good deal dislocated, some of the dips being to the N.W.; but beds of gritstone and purple shales, containing impure calcareous beds, are met with up to Mr. Gibsone's Great Permian Fault (which it would be better to call the Great South Fault), that brings in the Canobie thick or middle coal-field at Byreburn Foot, which is generally considered to represent the middle or thick coal-measures of Whitehaven, and the same strata at Common in Ayrshire. After passing over this coal-field, the limestone series of coal-measures is seen above the Hollows Bridge. As to the value of the seams in the last-named part of the coal-measures my observations did not allow me to form any opinion, except that they did not appear to be so rich in coal as the same strata are in Ayrshire and the West of Scotland. Mr. Ralph Moore estimates the Ayrshire coal-measures as follows:—

	Fathoms.
Upper coal-measures	313
Limestone series	52
Lower coal series	200 *

Now, in the Valley of the Ayr, near Catrine, there are from 250 to 300 fathoms to be added to the upper coal-measures, so as to connect the latter with the Ballochmoyle limestone; and in the Canobie section it has been previously shown that there are 100 fathoms of upper coal-measures above a similar bed of limestone; so, in estimating the distance down to the profitable coal-field at Canobie Bridge, some 350 to 400 fathoms will most probably have to be sunk through before that is

* 'Papers on the Blackband Ironstone of the Edinburgh and East Lothian Coal-field,' &c., by Ralph Moore, Mining Engineer, Glasgow, 1861, p. 9.

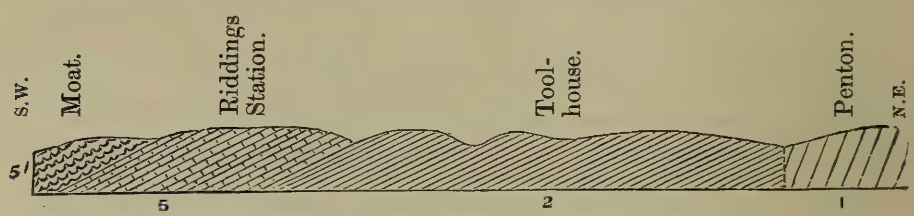
reached, assuming this coal-field to resemble that in Ayrshire, and that the upper and middle coal-measures in this district are conformable to each other.

Mr. Gibsone has fully described the profitable part of the Canobie coal-field in the memoir previously quoted; and, from his great practical knowledge of the subject, his opinion no doubt is of much value, and to be relied on. The point where I differ from him is the age of the red strata seen in the Esk, between Canobie Bridge and the Great Fault which brings in the profitable part of the Canobie coal-field near Byreburn Foot. He, like the mining engineers of the West of Scotland, classes these strata containing no beds of coal as Permian, whilst I term them upper coal-measures. My reasons for doing so are, that in their physical characters they are more like carboniferous than Permian deposits, and that they contain the Spirorbis limestone, *Stigmaria ficoides,* and other coal-plants. In former times, these fossil organic remains alone would have decided the age of the deposits; but, in Germany, that eminent geologist and palæontologist Dr. Geinitz, in his admirable work on the Permian beds, under the name of Dyas, does not hesitate to include beds containing the above-named fossil organic remains occurring in the Lower Rothliegende as belonging to the Dyas —his new term for Permian. When Permian and Triassic strata have been as much investigated as the coal-measures, we shall know more of their plants. To my surprise, Mr. Kirkham, a young geologist of Manchester, some time since showed me an undoubted *Sternbergia,* which he obtained from the Triassic Sandstone at Weston Point, near Runcorn; and several *Calamites* have been met with in the water-stones of Lymm, near Warrington; so the Triassic *Flora* may prove to be more allied to those of the Permian and carboniferous than at present supposed.

If we are to have a division between Permian and car-

boniferous strata (and, in the present state of our knowledge, most British geologists will probably consider that such a line of demarcation is convenient), there is no better evidence than the *Spirorbis carbonarius* and the *Stigmaria ficoides,* in the form of organic remains, to identify carboniferous strata by. When the latter fossil, with its rootlets, is found in shales, there can be no doubt that it grew on the spot where it is met with, and that it has not been drifted from a distance; but this would not be the case with a fragment of a specimen found in a sandstone, which might have been brought by currents of water and left in the locality where it is now found.

Moat and Penton Section. Distance 3½ miles.



Commencing with the Moat sandstone, as in the last-named section, and following the line of the North British Railway, a good view of the red clays, containing slight traces of veins of gypsum and thin bands of gritstone, is seen in the cutting the greater part of the way up to Riddings Junction Station. On the western bank of the River Liddel, below the station, is seen a small anticlinal axis of not more than 20 yards in length, which shows a bed of breccia 4 feet in thickness, lying between two beds of red sandstone. The breccia was composed of coal-measure sandstones, with some few limestones, cemented together by a red paste.

In the railway-cutting near the bridge over the Liddel, which carries the line to Canobie, the following section is seen on the line and in the cliff on the river-bank, in the descending order: namely,

	ft.	in.
Red shales	5	0
Soft red sandstone	4	0
Breccia	1	3
Purple shales	1	6
Green calcareous band	2	6
Red and variegated soft sandstone	4	0
Red shales, containing bands of gritstone, about	40	0

At the junction of the two lines of railway another small anticlinal axis is seen, dipping to the N.W. and S.E., and extending over 10 yards. Up to this point the strata appear to be Permian. Continuing the section along the line, a series of red and variegated shales, containing thin bands of gritstone, occur for a considerable distance, until we come to a brown sandstone marked with ripples, and having its lower portion mottled with red, similar to that at Knotty Holm, described in the last section. In the sandstone no fossils were met with; but the shales afforded *Stigmaria*-rootlets at several points in the railway-cutting between the Tool-house and Canobie Junction. In the flat piece of land near Penton, below the railway, at a place called Crooked Holm, a bore was made by the late Sir James Graham, Bart., some years since. By the kindness of Mr. Gibsone, some specimens of the limestone found in the bore were forwarded to me. All its characters reminded me of the Spirorbis limestone found at Canobie, and described in the last section; but no fossil organic remains were found in the specimens submitted to me. The following is a section of the

Inch Bore.

	fath.	ft.	in.
Sand and gravel	1	2	0
Brown-red sandstone	2	2	5
Grey sandstone, in thin layers	6	3	11
Brown clay	1	2	0
Sandstone	3	1	9
Carry forward	15	0	1

	faths.	ft.	in.
Brought forward	15	0	1
Brown clay and shale	2	4	6
Red clay	1	2	6
Light clay	1	2	6
Sandstone	2	1	9
Red clay	2	4	6
Sandstone	0	2	0
Brown and red clays and sandy shales	13	2	2½
Soft grey sandstone	7	2	8
Red sandstone	7	0	7
Red and brown clay and shales	11	1	2
Red shale and gypsum	2	3	0
Blue and white clay	0	3	10
Limestone (ft. in.: Red ... 3 5½; White. 3 10; Clay ... 2 0; Red ... 0 7)	1	3	10½
Brown shales, spotted with green in places	5	2	8
Brown sandstone	0	4	2
Brown shale, spotted with green in places, and containing gypsum in red clay and limestone-nodules	11	3	8¾
Soft brown sandstone, mixed with white	2	0	8
Brown clay and shale	3	0	3
Hard grey sandstone	0	3	1
Brown and light-coloured clay	1	0	7
Hard grey sandstone	0	0	8½
Grey shale	0	1	3
Hard brown sandstone	0	2	3
Purple and brown shale	1	4	7½
Light grey shale	1	2	6
Light grey shale and clay	0	5	9
Brown and grey sandy shale	2	3	9
Hard brown sandy shale	1	3	3½
White purple and yellow clay	0	1	9
Dark brown clays and shales	3	3	8
Dark blue clay	0	5	5
Light blue and brown clay	2	0	4
White brown and yellow clay	0	2	6
Hard sandstones, blue, white, and brown, divided by deep-red sandy clay in two places, with white and blue clay	7	5	8¾
Brown and blue shale	2	0	11½
White and brown sandstone	2	1	0¾
Carry forward	122	5	10¼

	faths.	ft.	in.
Brought forward	122	5	10¼
Blue clay and sandy shale	1	2	4½
White and blue sandstone	1	1	11¾
Black-brown clay and white sandstone	1	3	3¼
Blue and white shale and sandstone	2	5	10½
Shale in thin layers, mixed with light blue clay	2	4	1
White, red, and yellow coarse sandstone	1	4	4¾
Light blue clay	0	2	0
Brown clay	0	2	0½
Variegated clays of white and brown colours	0	4	3
Brown sandstone	0	5	0¼
Brown and peuce-coloured sandstone	0	4	6½
Brown and blue sandy shale and hard brown sandstone	1	4	8
Brown limy sandstone	0	2	2¼
Sandy clays	0	4	10½
Brown sandstone, clays, and thin ribs of dark blue stone, containing iron and lime	0	5	1½
Fathoms	141	2	6¾*

Mr. Matthias Dunn, Government Mine-Inspector, in a paper on the coal-fields of Cumberland, and on the probability of coal being found under the New Red Sandstone which surrounds Carlisle, printed in vol. viii. of the 'Transactions of the North of England Institute of Mining Engineers' (p. 141), says, "In the years 1857–58 a boring was made adjoining the River Liddel, in the lands of Sir James Graham, Bart., under the management of Mr. Gard, from Cornwall, by means of his patent instrument, worked by a steam-engine, the result of which was unsatisfactory in many respects, both as to the depth bored and the imperfect manner of accounting for the strata passed through. There is also reason to believe that he was in very troubled ground. A copy is hereto annexed of the boring, which was given up at the depth of 56 fathoms, the contractor being quite dispirited.

* Transactions of the North of England Institute of Mining Engineers, vol. xi. p. 79.

Sections of borings at the Inch, Netherby property, up to 6th September, 1858.

	faths.	ft.	in.
Open cutting	2	3	0
Sandstone and clay	1	5	6
Hard shaly rock	0	1	9
Sandstone	0	0	6
Clay	0	0	6
Softstone	0	2	0
Hard clay and sandstone	1	1	0
Clay	1	0	0
Layers of clay and sandstone	4	4	10
Hard sandstone	2	3	8
Soft sandstone	3	2	9
Sandstone	0	4	0
Soft clay (Large streams of water occurred)	4	3	6
Running sand (Large streams of water occurred)	0	4	0
Ironstone (Large streams of water occurred)	0	0	1
White clay	0	3	0
Grey sandstone	0	1	6
Purple sandstone	0	2	0
Purple sandstone, soft	0	4	0
Hard sandstone	0	3	0
Blue clay	1	2	0
Blue clay, lighter colour	0	1	8
Grey sandstone	1	0	0
Clay-slate	0	3	0
Clay-slate, blue	0	1	0
Shaly rock	0	5	0
Shaly rock	0	4	0
Hard grey freestone	10	4	0
Red limestone	0	0	8
Grey freestone	3	0	0
Soft shaly rock	3	1	0
Mottled clay	4	0	0
Brown freestone	3	4	0
Limestone	0	1	6
Mottled freestone	0	2	6
Fathoms	58	0	11

"The reason why this position was selected was its contiguity to the colliery of His Grace the Duke of Buccleuch at Canobie, in Dumfriesshire, which possesses the following workable seams of coal, viz.:—

	faths.		Thickness. ft.	in.
At	23	Three-quarter coal	3	6
,,	40	Main coal	6	0
,,	54	Nine-feet seam	9	0
,,	55½	Three-feet seam, sunk to in engine-pit	3	0
,,	65½	Five and a half feet	5	6
,,	76½	Five feet	5	0
,,	79	Seven feet	7	0
,,	82	Three feet	3	0

"The ancient workings of the colliery were at Byreburn, to the north-eastward of the present colliery, upon a lower series of coals there cropping out, viz. a seam of 6 feet, another of 3 feet 8 inches, and a third of 2 feet 4 inches, all of which seams undoubtedly underlie those of the present colliery.

"At about 190 yards south-east of the (Canobie) engine-pit, this coal-field is interrupted by a downcast brow of red sandstone; and on the east side the coal terminates at the mountain limestone."

Copies of the above bores are given to show what information has been obtained in the searches for coal. The strata gone through are not very easily recognizable from the journals, but they both appear to me to have been made in the same neighbourhood, and to be wholly through upper coal-measures, most probably under the Knotty Holm sandstone, as the arenaceous beds found in the upper portions of both of them bear some resemblance to that rock. The thin bed of Spirorbis limestone is not noticed. The beds of limestone alluded to are like the calcareous beds seen in the lower portions of the upper coal-field in the Esk, at Canobie, before we reach the fault which brings in the profitable part of the Duke of Buccleuch's coal-field. The parties, as Mr. Dunn states, considered the last-named bore to be unsatisfactory, probably because no coal-seams were found; but if the Canobie and Penton upper coal-

measures bear any resemblance to the same strata in Ayrshire, on the banks of the Ayr, a greater depth will most probably have to be gone through before the profitable middle coal-field is reached, as previously stated in my remarks on the Canobie section.

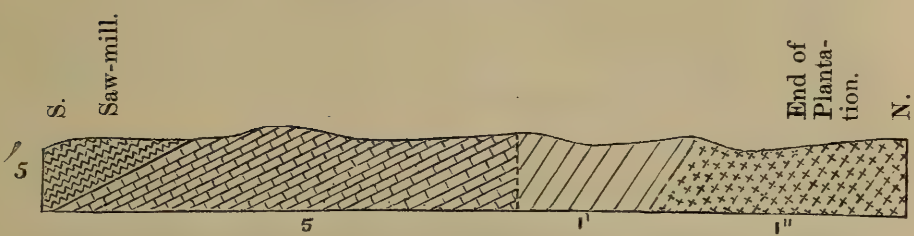
The dip of the strata near the Tool-house on the railway, taken in the brown sandstone, was to the N.N.W., at an angle of 25°. The red strata in the banks of the Liddel, below the railway, also dipped to the N.N.W. Near the bridge over the railway at Penton is a fine section, showing the fault, which brings in a coarse sandstone, very like a millstone-grit, and coal-measures containing 4 small seams of coal, one of which had a gannister floor. The dip of these strata is to the east, and they belong, as Mr. Gibsone describes them, to the millstone-grit series of coal-measures. Under these strata, in the Liddel, above the old bridge, is seen a seam of coal 6 inches in thickness, with a gannister floor, and a bed of impure limestone containing crinoidal columns for a roof, all dipping slightly to the east.

The great fault above described, which brings the brown sandstone and the millstone-grit into contact, is most probably the same as that I have noticed in the Esk, near Byreburn Foot, as there also bringing in the red measures. At Penton I consider the red-coloured strata to be upper coal-measures, as they contain *Stigmaria*-rootlets, like similar strata above Canobie Bridge, and not Permian beds as Mr. Gibsone has described them to be.

In this section we have not only failed to see the two beds of breccia and the Spirorbis limestone, but also two thick red sandstones of a soft description, seen in the Long Pool and below the Manse at Canobie. These deposits could not have escaped observation if they had been present; and their absence can only be accounted for by the Permian strata to the north of Riddings Junction being thrown

down by a fault, and the upper coal-measures thus coming in and replacing such strata.

Section from Sir F. Graham's Saw-mill to the end of the Carlinway Plantation, below the Tilery. Distance, $1\frac{1}{4}$ mile.



About half a mile to the south-east of the Moat Quarry, mentioned in the last section, is a small valley running nearly parallel to that of the Liddel, in which flows a stream known by the name of the Carlinway Burn, that joins the Esk above Netherby. It commences at the saw-mill belonging to Sir Frederick Graham, just below which is seen in the brook a dark red sandstone, thin-bedded and ripple-marked, in all respects similar to that seen at Moat, of which, no doubt, it is a continuation. The dip of this stone is to the W. at an angle of 8°. For a distance of about three-fourths of a mile up the brook, red shales and sandstones are seen dipping slightly to the west until we reach a bed of breccia, composed of red and white sandstone and limestone in a red paste, having a thickness of 7 feet, resting on a variegated red sandstone. The dip of these strata is to the N.E. at an angle of 18°. The disturbance which has caused this change of dip cannot be clearly seen. For a distance of about 100 yards above the bed of breccia the strata cannot be traced; and when they do make their appearance in the form of soft red clays, their position is difficult to determine, but most probably they incline to the north-east. Again, for about 100 yards the strata are covered by alluvium. Then

appears a sharp-grained sandstone, of a drab colour, mottled with white spots, about 25 yards in thickness, which dips to the W. at an angle of 45°. The strata, consisting of fine-grained sandstones and red limestones, containing common mountain-limestone shells and corals, occupy the valley to the end of the wood, increasing their dip to a vertical position, when they disappear under a covering of drift. It appeared to me that the strata had been affected by a fault running from S.W. to N.E.

In this section, only Upper Permian sandstone, red shales and breccia, and lower carboniferous gritstones and limestones make their appearance, the upper and middle coal-measures not being seen at all.

Mr. Gibsone, in his paper before quoted, considers these highly inclined sandstones and limestones not to be of Carboniferous age, and he designates them as his Permian Magnesian Limestone series. No doubt it is frequently difficult in Cumberland to distinguish a Permian limestone from a carboniferous one, in the absence of fossil organic remains; but in the case of these Carlinway Burn beds no such difficulty exists; for, in addition to common carboniferous Crinoids, there is plenty of the *Producta giganteus* and other well-recognized species of carboniferous fossils. The Permian beds in this section appear to be brought into juxtaposition with the millstone-grit by a great fault, similar, but not the same, to that already noticed in the Canobie and Penton sections; but there is no appearance of the upper coal-measures as seen in both those sections, so far as my observations enabled me to judge.

The Eden and Caldew Sections.

In the bed and along the banks of the River Eden, in the vicinity of Carlisle, a deposit of several hundred feet in thickness of red and variegated marls, parted by bands

of water-stone and thin gritstone, is seen. These strata have a general inclination to the west, but the beds are frequently seen contorted. Under the Silloth Railway bridge they dip to the W. at an angle of 13°. Between this place, at the Lias at Oughterby and Quarry Gill, on the north-west, and the false-bedded sandstone Trias of Rockliffe and West Linton to the north, and How Mill to the east, little can be seen of the underlying strata; and, up to this time, no evidence has reached me of the Lias having been met with to the east of the River Eden; so it is probable that the Triassic marls occupy the district until the sandstone makes its appearance.

Mr. Dunn, one of Her Majesty's Inspectors of Collieries, states that Mr. Cockburn, of Allenwood Paper-Mill, not far from How Mill, bored in the red sandstone to the depth of 600 feet, and found the rock hard and tight throughout, with very little water. There did not seem to be any change in the strata, either as to colour or the nature of the rock, from the commencement to the close of their operations *.

At and near Carlisle, the Triassic marls are well known to be underlain by a soft red sandstone, like that previously described, and exposed in the valley of the Caldew at Holmhead. Most of the deep wells in the city are sunk through the marls to reach the water generally found in the underlying sandstone. At the pumping-engine for the canal by Edenside, immediately above the red and variegated marls, there was a section in the pump-well which distinctly showed the marls on the top gradually passing down into the red sandstone below †.

As we proceed up the Caldew from its junction with the

* "On the Coal-fields of Cumberland," by Mr. M. Dunn, 'Transactions of the North of England Institute of Mining Engineers,' vol. viii. p. 154.

† For this and other information relative to the marls and red sandstone of Carlisle, I am indebted to the kindness of Mr. Richard B. Brockbank, one of the partners of the firm of Messrs. Carr and Co.

River Eden, the red and variegated marls are soon perforated, and the Holmhead sandstone met with. This was the case at the Gas-Works. At Messrs. Carr and Co.'s works in Caldewgate, some years since, a bore-hole was put down to the depth of 180 feet below the bottom of the well, and all the distance was in soft red sandstone, with the exception of a shaly clay, termed by the well-sinkers quicksand. The same rock has been proved all the way to Messrs. Fergusson's mill at Holmhead, near which place it is seen in the banks of the Caldew, dipping to the W. at an angle of 10°.

In a bore-hole made by Messrs. Fergusson at Holmhead some years since, the following section was met with:—

	feet.
Clay (drift)	9
Soft white sandstone	108
Soft red sandstone, gone into, but not through	117
	234

Further up the valley, in the bank above the rifle-butts, the sandstone is seen dipping to the N.N.W. at an angle of 12°. It is met with in the valley, a mile further up, just before we reach the village of Dalston, dipping in the same direction, and at about the same angle. Above Dalston, at the weir across the Caldew, a similar sandstone is found, with a dip to the N.N.W. at 12°. All the sandstones seen in the valley of the Caldew, up to this point, are similar in appearance, and it is difficult to separate one from another.

Below the farm-house occupied by Mr. Carlisle, a little further up the valley, a quarry of thin-bedded and ripple-marked sandstone of a deep red colour, which has been used for building-purposes, is seen. This stone dips to the N.N.W. at an angle of 20°, and it reminded me more of the Shawk sandstone than any which had hitherto come to my notice. I could not see the red clays and beds of soft

red sandstone and breccias lying under it, nor could I trace its junction with the red sandstone seen at the weir below; but it appeared to me to be most probably of Permian age. Further up the river, opposite Holm Hill, is seen a thin-bedded sandstone of a bright red colour, dipping to the S.W. at an angle of 12°. A little further up the Caldew from this point, that river is joined by a brook known by the name of the Raw Beck; and about 100 yards above the confluence of these streams, just below Gatescale, is a most interesting section of upper coal-measures, in which occurs a bed of porcelain limestone, of mottled grey and liver colours, containing *Spirorbis carbonarius* and a *Cypris*?, in all respects exactly similar to the limestones of Ballochmoyle and Canobie, previously described by me.

In a distance of about 1000 yards above the limestone was a series of upper coal-measures, consisting of fire-clays containing *Stigmaria ficoides*, liver-coloured shales and gritstones, and a thick bed of soft red sandstone, which dipped to the N.W. at an angle of 18°. These strata are suddenly interrupted by the occurrence of a thick bed of red and variegated sandstone, false-bedded, of coarse grain, occasionally coloured by black oxide of manganese, much jointed and incoherent. At the bridge over the Caldew, leading to Raughton Head, it dips to the N.N.W. at angles varying from 20° to 25°. It continues up the valley all the way to Stockgillwraith Bridge, where it has been quarried for building purposes. It is there of a flaggy character, and dips gently to the west. It must be of great thickness, and it will most probably be proved to be of the age of the Permian soft red sandstones, but I had not sufficient evidence to enable me to be certain on that point.

The beds of the upper coal-measures, as seen in the Raw Beck, occurred in the following (descending) order: viz.,

	yds.	ft.
1. Red and variegated clays	13	0
2. Bed of limestone containing *Spirorbis*, &c.	0	1
3. Red clays	10	0
4. Purple shales containing *Stigmaria*-rootlets	80	0
5. Soft red sandstone	40	0
6. Purple shales	16	2
7. Shales, sandstones, and fire-clays, chiefly of a purple colour, about	150	0
	310	0

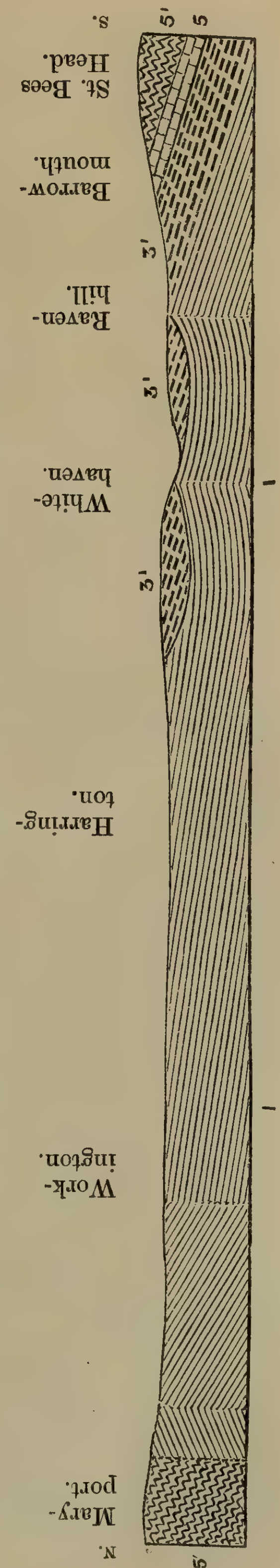
In the Shawk section*, the Permian strata are brought into juxtaposition with the mountain limestone; so it is probable that the fault which passes through Westward Chapel and Shawk, and brings in the latter rock in those places, extends up to the Caldew, and brings in the carboniferous beds last described.

The occurrence of upper coal-measures in this part of Cumberland has not yet, so far as I can learn, been noticed in any publication, and is an interesting fact. To my mind it tends to show that there is only the middle part of the coal-measures exposed at Whitehaven and other places on the west side of Cumberland, whilst at Canobie and Raw Beck we have the upper coal-measures—thus indicating something like a synclinal axis in the valleys of the Solway and Eden, the upper coal-measures of Canobie, and the Permian strata of Canobie and Moat,—the soft red sandstone (Trias) of Longtown, West Linton, and Rockliffe dipping to the south, covered by the red and variegated marls of Carlisle, and the Holmhead and Dalston sandstone, the sandstone seen in the Caldew below Holm Hill, and the upper coal-measures of Raw Beck dipping to the north, as shown in the accompanying woodcut.

It is desirable that these upper coal-measures should be proved, by careful boring, as to whether they are underlain by a portion of the middle or profitable coal-field, like that of Whitehaven, or whether they are an unconformable part

* Vol. xiv. p. 117 (2nd Series) of the Society's Memoirs.

Section from Hollows, near Canobie, to Holmhill. Distance, 20 miles.



Section along the Coast, from Maryport to St. Bees. Distance, 17 miles.

of the upper coal-measures, resting on the mountain-limestone series without the intervention of the middle coal-measures, which it is possible may be the case if one portion of the coal-measures is sometimes unconformable to the other, as has been recently shown to occur in the South Staffordshire coal-field by Mr. Scott.

Maryport and St. Bees Section.

From the neighbourhood described in the last section, by way of Shawk, Howrigg, Westward Chapel, West Newton, Allonby, to Maryport, the upper sandstone of the Permian ranges and bounds the northern part of the coal-field from Aspatria to Maryport. At the latter place, on the beach to the north of the town, is Bank End Quarry, a red-sandstone, fine-grained, laminated, and ripple-marked, exactly resembling the Shawk, Howrigg, Westward, and St. Bees sandstones, and in no way to be distinguished from them. It dips to the north-east at an angle of 17°, and by the practical coal-masters of the district this sandstone is considered to lie in a great downthrow of the coal-measures, and coal has never yet been reached under it. To the east, about a mile on the railway to Aspatria, at Birkby Mill, a good section of the mountain-limestone series of coal-measures, with a coal of 8 inches, having a Gannister floor, is seen. The strata dip to the south-west at 12°, and underlie the Maryport coal-field*.

In the neighbourhood of Maryport, Mr. Wallace, an extensive coal-proprietor there, informed me that the red sandstone of Whitehaven had been sunk through, and coals worked under it. This rock, however, was not seen by me

* Two other patches of carboniferous limestone, the one at Distington, adjoining the Harrington coal-field, and the other at Hensingham, adjoining that of Whitehaven. The close proximity of the carboniferous limestone to this part of the Cumberland coal-field appears to show that the latter belongs to the middle coal-measures, and the millstone-grit series is there of no great thickness.

in the neighbourhood. So far as my observations went, the middle coal-field extended south along the coast, from Maryport by Workington and Harrington, to Lord Lonsdale's quarry of red sandstone to the north of Whitehaven. This sandstone is of a remarkable character, and is evidently the same as the rock seen to the south of Whitehaven and on to Barrowmouth. Its thickness must be about 140 feet, and the upper portion of it might be taken as a coal-measure rock; indeed it was considered as such by me when I saw it many years ago. Then I had only examined the upper portion of it, as exposed in the coast-section at Barrowmouth, and had not seen the lower parts, which for 30 feet are of a conglomerate character, containing white quartz pebbles of the size of a common bean and much peroxide of iron and decomposed felspar, and not to be distinguished from millstone grit,—altogether different in character from the upper part of the rock, and containing traces of what appears to me like volcanic ash.

Prof. Sedgwick, when he first noticed this rock, described it as lower red sandstone; and Mr. Bourne and the other local geologists of the district designate it by that name, although the latter appear to think it conformable to the underlying coal-measures. Its dip is nearly level, but inclines slightly to the south. It contains common coal-plants of the genera *Sigillaria, Calamites, Sternbergia,* and *Dadoxylon,* the specific characters of which cannot be made out. To the south end of the quarry, near the colliery, 10 feet in thickness of coloured clays, of a purple colour, make their appearance under the sandstone, and dip towards the north; probably they may be brought in by a fault, as the red sandstone is not seen on the hill above the colliery. To me it appeared as if the coal-measures were there unconformable to the overlying red sandstone. In the valley occupied by the town of Whitehaven the strata are not well exposed; but to the south of the town the thick

red sandstone last described is again seen capping the coal-measures, and forming an anticlinal axis at Ravenhill, dipping from that point to the north towards Whitehaven, and to the south towards Barrowmouth.

At Ravenhill the lower portions of the sandstone are quite of a conglomerate character, and contain many white quartz pebbles and one large slate-pebble (6 inches in diameter), as well as a considerable quantity of peroxide of iron and decomposed felspar, which appeared to me like volcanic ash. The dips of the sandstone and the coal-measures appeared to be about the same; but, so far as my observation went, there was no appearance of the passage of one rock into the other, but only a simple superposition. The lower portions of this sandstone are exactly of the same character as the sandstones of Astley and Bedford, described by me as Lower Permian, and in no wise to be distinguished from a similar coarse sandstone (containing coal-plants) seen in the ballast-quarry at Moira near Ashby-de-la-Zouch, which Mr. Woodhouse and other practical geologists of the neighbourhood consider to be quite unconformable to the underlying coal-field. This rock, if not to be classed as Permian, must be taken as upper and unconformable coal-measures; for in Lancashire and Leicestershire it is quite unconformable both to Permian strata above and coal-measures underneath. The chief reason which has induced me to remove them from the carboniferous strata is the conglomerate character of the lower part of the sandstone, which, as previously stated, is more like a millstone-grit than an upper coal-measure rock. For the present it appears to me desirable to retain it, as Prof. Sedgwick first designated it, by the name of Lower Red Sandstone, or my name of Lower Permian. It is to be remarked, that when the soft sandstone of Collyhurst, Kirkby Stephen, and Hilton is absent this sandstone is generally met with; and, so far as my know-

ledge extends, it has not yet been found at a distance from profitable coal-fields. It is absent in all the Permian sections that I have seen in Yorkshire, Westmoreland, and Cumberland, from Westhouse to Penton and Canobie.

The section of Barrowmouth now claims our attention. When this interesting coast-section was visited by me many years ago, my chief attention was directed to the magnesian limestone and the red marls; and the magnificent rock of Upper Permian sandstone above, or the thick lower or Whitehaven sandstone underneath, did not claim much of my observation, my object then being chiefly to draw attention to the magnesian limestone and the underlying conglomerate.

The following is a general section of the Permian strata as exposed in the cliff-section, and their estimated thicknesses in a descending order:—

	ft.	in.
1. Fine-grained red sandstone, laminated and ripple-marked, same as that seen at Moat, Cove, Shawk, Westward, Maryport, and other places, which may be conveniently called St. Bees sandstone, fully	1000	0
2. Red shaly marls	30	0
3. Red marls, containing granular gypsum	29	1
4.*Magnesian limestone of a cream-colour, containing shells of *Bakevellia* and *Schizodus*	10	6
5. Breccia, containing pebbles of coal-measure, sandstone, and slate-rocks	3	0
6. Red and purple sandstones	110	0
7. Conglomerate sandstone, full of white quartz pebbles, and containing common coal-plants	30	0

The bed of limestone contains numerous small hollows filled with spar, and is one mass of indistinct fossil shells, chiefly of the genus *Bakevellia*. Its composition in 100 parts is as follows:—

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* Above the bed of limestone, Prof. Sedgwick noticed a bed of siliceous sandstone, containing jasper and chalcedony, which I did not see, probably owing to its being covered up by fallen *débris*.

Lime	43·20
Magnesia	5·53
Carbonic acid	40·69
Protoxide of iron	1·14
Alumina	4·25
Silica	5·19
Specific gravity 2·62	100·00*

The Barrowmouth section is remarkable for the absence of the soft red sandstone of Kirkby Stephen, Belah, Hilton, and Canobie, which ought to underlie the breccia. With this exception, it is the most complete Permian section to be met with in the north-west of England that has yet come under my observation. The St. Bees sandstone is seen to pass down into the red shaly clays on the hill-side at Barrowmouth, and in its range southward extends across the country by Bolton Wood, south of Cleator, Egremont, Calder Abbey, Gosforth, to Drigg Cross, and the district lying between those places and the sea. Prof. Sedgwick thought that it could be traced southward into Furness. It is generally found as a fine-grained and laminated sandstone of a red colour, although some of its beds are of a drab colour, and it is in general use throughout the country as a building-stone. Some of its laminated beds, which contain numerous fine plates of mica, are also used for slates and flags. It contains beautiful ripple-marks, but in the neighbourhood of St. Bees it has up to this time, to my knowledge, yielded no footprints of animals. None of the deposits of hæmatite iron, so far as I can learn, have ever been found in this rock. In the upper parts of the rock, at Fleswick and Seacote, the colour is of a brighter red, and the beds are thicker and not so much laminated as they are in the lower parts. From Barrowmouth to Seacote the sandstone has a southerly dip, averaging about 9°, and the distance is three miles; so the

* For this analysis I am indebted to the kindness of Mr. M. Binney, of Bathgate.

thickness of the rock, assuming there are no faults, cannot be less than 1000 feet.

At Barrowmouth, as noticed by Prof. Sedgwick, the Permian strata are thrown up by a little fault to the north. This fault is traced through the Earl of Lonsdale's Croft Pit at Preston-Hows, and probably afterwards seen in Ben How Quarry.

By the kindness of Peter Bourne, Esq., mining-engineer to the Earl of Lonsdale, I am enabled to give the section of the Permian and carboniferous strata met with in sinking the Croft Pit, a short distance inland from Barrowmouth:—

An Account of the Strata in Croft Pit, at Preston-Hows, situated about 1½ mile to the south-west of Whitehaven. The divisions and remarks on the right-hand side are my own, and were not in the original.

		Description of Strata.		Thickness of each Stratum.	
		No.		ft.	in.
Permian.			Soil	1	3
			Soil and clay mixed	4	9
			Black soil	1	0
	Beds of Gypsum	1.	Brown soft limestone, resembling stone mail, in irregular strata	9	0
		2.	Dark-coloured limestone, harder	6	0
		3.	Yellowish limestone, mixed with spar	4	0
		4.	Reddish hard limestone	2	0
		5.	Reddish hard limestone, but with finer particles	1	6
		6.	Hard dark-coloured limestone	1	4
		7.	Yellowish limestone, mixed with spar	4	0
		8.	Soft brown limestone	4	2
		9.	Soft brown and yellow limestone, mixed with freestone	2	6
		10.	Limestone, mixed with yellow freestone	2	0
	Conglomerate.	11.	Reddish soft freestone	1	6
		12.	Red slate, striated with freestone in thin layers	2	6
	Whitehaven Sandstone.	13.	Red freestone	42	6
		14.	Soft red slate	0	6
		15.	Red slate, striated with red freestone in thin layers	25	0
		16.	Red slate striated with freestone	27	0
		17.	Strong red freestone, rather greyish	29	9
		18.	Lumpy red freestone, speckled with white freestone	0	9
			Carried forward	173	0

An Account of the Strata in Croft Pit (*continued*).

		Description of Strata.	Thickness of each Stratum.	
		No.	ft.	in.
		Brought forward	173	0
	Middle Coal-measures.	19. Blue argillaceous schistus, speckled with coal	0	9
		20. Red soapy slate	13	0
		21. Black slate, with a small appearance of coal under it	1	0
		22. Ash-coloured, friable, argillaceous schistus	4	6
		23. Purple-coloured slate, striated with freestone	23	3
		24. The same, and under it black slate; the thickness of each not distinguished	4	0
		25. Coal, 1st	1	0
		26. Soft whitish freestone	10	2
		27. Blackish slate, a little inclined to brown	4	11
		28. Coal, 2nd	1	10
		29. Blackish slate, intermixed with coal	2	6
		30. Whitish freestone	8	6
		31. Strong bluish slate, mixed with grey freestone	3	0
		32. White ironstone	1	0
		33. Freestone, striated with blue slate	1	8
		34. White freestone, striated with slate in thin layers	9	3
		35. Dark-blue slate	13	6
		36. Coal, 3rd	0	9
		37. Dark grey shale	15	8
		38. Coal, 4th, with a mixture of slate about one inch thick	2	0
		39. Grey freestone, mixed with ironstone	8	0
		40. Hard white freestone	15	6
		41. Coal, 5th	1	0
		42. Shale, mixed with freestone	8	0
		43. Olive-coloured slate, adhering to black slate, superincumbent on coal	2	4
		44. Coal, 6th	1	1
		45. Black shale, mixed with freestone	8	8
		46. White freestone, mixed with slate	8	0
		47. Dark-blue slate	22	4
		48. Coal, 7th	1	3
		49. Black shale, mixed with freestone	7	6
		50. Strong white freestone	6	0
		51. Brown ironstone	3	0
		52. Dark-grey slate	6	0
		53. Dark-grey shale, with an intermixture of Coal, 8th, about 5 inches thick	5	6
		54. Light-coloured slate, mixed with freestone	5	6
		55. Blue slate, striated with freestone	10	0
		56. Strong white freestone, a little tinged with iron	2	6
		Carried forward	417	5

An Account of the Strata in Croft Pit (*continued*).

	Description of Strata.	Thickness of each Stratum. ft.	in.
	No.	ft.	in.
	Brought forward	417	5
Middle Coal-measures (*continued*).	57. Very black shivery slate	10	3
	58. Strong Coal, of a good quality, 9th	0	4
	59. Soft grey slate	0	3
	60. Very black Coal, 10th; burns well	0	8
	61. Hard black shale	1	7
	62. Coal, mixed with pyrites, 11th	1	2
	63. Argillaceous schistus, grey and brittle	3	0
	64. Blue rough argillaceous schistus	4	6
	65. Fine blue slate	3	0
	66. Freestone, mixed with ironstone	3	0
	67. Black shivery slate	6	0
	68. Dark-blue slate, very fine	5	6
	69. Dark-blue slate, very brittle	0	6
	70. Coal, 12th	2	6
	71. Soft grey argillaceous schistus	0	6
	72. Argillaceous schistus, mixed with freestone	2	0
	73. White freestone, with fine particles	7	0
	74. Blue slate, striated with white freestone	4	7
	75. Light-blue slate, very fine	3	0
	76. Blue slate, a little mixed with ironstone	12	0
	77. Black shivery slate	1	0
	78. Coal, 13th	0	6
	79. Brownish hard slate	9	0
	80. Strong blue slate, tinged with ironstone	28	6
	81. Dark-blue slate, rather inclined to brown, of a brittle nature	1	6
	82. Blue soft brittle slate	0	6
	83. Coal, 14th	0	6
	84. Lightish-grey brittle soapy argillaceous schistus	4	0
	85. Freestone, striated with blue slate	7	0
	86. Fine blue argillaceous schistus, striated with white freestone	4	0
	87. Black slate, with hard, sharp, and fine particles	3	0
	88. Very light-blue slate, remarkably fine	27	0
	89. Coal, 15th	5	4
	90. Soft grey argillaceous schistus	4	3
	91. Black shivery slate	2	2
	92. Coal, 16th	1	3
	93. Strong lightish-coloured shale	3	4
	94. Blue slate, striated with white freestone	3	4
	95. Ironstone	0	4
	96. Grey slate	3	9
	97. Strong white freestone	5	6
	98. Freestone, striated with blue slate	0	10
	99. White freestone	1	3
	100. Freestone, striated with blue state	3	11
	Carried forward	611	0

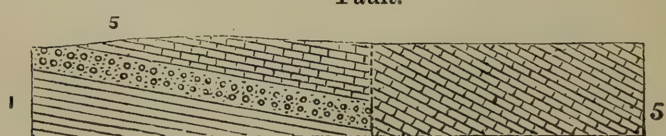
An Account of the Strata in Croft Pit (*continued*).

		Description of Strata.	Thickness of each Stratum. ft.	in.
		No.	ft.	in.
		Brought forward	611	0
		101. Black slate	0	5
		102. Freestone, striated with blue slate	1	4½
		103. Strong white freestone	0	4
		104. Freestone, mixed with blue state in thin layers	2	4
		105. Strong white freestone	0	5
		106. Greyish slate, of a shivery nature	6	0
		107. Freestone, mixed with blue slate in thin layers	4	0
	Middle Coal-measures (*continued*).	108. Very strong white freestone	5	3
		109. Fine blue slate	2	3
		110. White freestone, striated with blue slate	0	7½
		111. Fine blue slate	0	4
		112. White freestone, striated with blue slate	2	1
		113. Freestone, striated with blue slate in fine particles	0	10
		114. White freestone, in thin layers	0	4
		115. White freestone, in thin layers, but more friable	0	5
		116. Fine blue slate	2	1
		117. Coal, 17th	7	10
			647	11

This section differs in several respects from that previously given, and as seen in the coast-section at Barrowmouth, especially in the omission of the bed of gypsum, which evidently has been mistaken for limestone; and the beds of limestone are more numerous and thicker inland than on the coast, as is shown by the much greater thickness of the limestone at Ben How to that at Barrowmouth.

Ben How Section.

Fault.



At the Ben How Quarry, by the roadside leading from Whitehaven to St. Bees, is a beautiful section of the mag-

nesian limestone, and the underlying breccia first described by Prof. Sedgwick. The St. Bees sandstone has outcropped before it reached the quarry, and no trace of the Whitehaven sandstone is seen. There are two quarries, one on the east and the other on the west side of the road. In the first-named the small upthrow is seen, which brings in the strata to the north, as noticed at the Croft Pit and Barrowmouth; and in the last-named is the greatest thickness of limestone exposed.

The strata here occur in the following descending order:—

	ft.	in.
1. Hard cellular limestone, about	30	0
2. Breccia, composed of angular pieces of slate, white quartz, mountain limestone, and red sandstone, cemented together by a red paste	8	0
3. Loose pebbles in sand	1	0
4. Strata not well seen	8	0
5. Red sandstone	2	0
6. Red and liver-coloured shales	30	0
7. Coal	0	6

The Permian strata on the high or south side of the fault dip to the S.W. at an angle of 28°. The beds on the low or north side of the fault dip to the S.E. at an angle of 12°; and the underlying carboniferous strata dip to the S.W. at 18°. The Croft Pit and Barrowmouth bear due west, about a mile from this quarry. The most remarkable features observed in this quarry are the absence of the soft red sandstone of Kirkby Stephen and the thick red sandstone of Whitehaven, which latter rock is so well seen at Barrowmouth, and was met with in sinking the Croft Pit.

The Whitehaven coal-field appears to belong to the middle coal-measures, and to resemble the Canobie and Ayrshire fields rather than that of Newcastle. The Whitehaven sandstone overlies this coal-field; and no trace of upper coal-measures has yet been met with in the district.

This singular rock, from its conglomerate character, evidently points to the action of stronger currents of water than those from which the fine sedimentary coal-measures were deposited, and a mixture of something like volcanic ash indicates disturbances of the crust of the globe; but the plants contained in it are, so far as their characters can be deciphered (for they are not in a good state of preservation, and appear more like drifted specimens*), ordinary coal-plants. It is quite unconformable to the overlying Permian breccia at Barrowmouth, which is seen lying in hollows worn out of it. No doubt it is seen superimposed on the coal-measures near Whitehaven, and probably has there a similar inclination; but it is not on middle but upper coal-measures where we should expect to find it, as is the case near Manchester, where it is found over the Four-feet Mine in the upper coal-series. If it be an ordinary carboniferous sandstone, conformable to its underlying coal-measures, it will be found in a constant position, and not varying from place to place. It is quite true that we may account for its position by assuming it to be an unconformable portion of the coal-measures; but if we still cross the boundaries of conglomerate character and unconformable position and take no notice of these features, what is to stop us from classing the whole of the Permian beds as upper coal-measures? This sandstone appears to me to occupy the position of the Lower Rothliegendes of Germany; and if they are to be retained as Permian, it ought to be. On the other hand, if they are to be classed merely as unconformable coal-measures, it must follow the same lot. Not having examined the Permian deposits of the Continent, I am unable to give any further opinion

* It is probable that these plants are a part of the carboniferous flora which have been drifted from their habitats and mingled with the débris of the Lower Permian. This was the opinion of Prof. Sedgwick many years since; and, if I am not mistaken, my friend Prof. Morris, F.G.S., inclines to the same opinion now.

than that afforded by their fossil remains; and these, so far as plants and fishes are concerned, do not greatly differ in the lower beds more than those found in one part of the carboniferous strata vary from those met with in another portion.

Concluding Remarks.

The occurrence of an upper coal-field and its characteristic Spirorbis limestone in the south-west of Scotland and the north-west of England, as shown in this paper, taken in connexion with similar beds at Ballochmoyle Braes, in Ayrshire, as well as the fragments found in the conglomerate of Westhouse, near Kirkby Lonsdale, all tend to prove the former occurrence of these beds over a large area, and their probable removal by denudation from the West Cumberland coal-field prior to the deposition of the Whitehaven sandstone; for it is certain that up to this time no traces of these strata have been met with in that district, although the Permian strata are well exposed; and they were likely to be noticed, if present.

So far as my knowledge extends, there has yet appeared no notice of these coal-measures having ever been found in the east of Scotland, the north-east of England, Yorkshire, Derbyshire, or Nottinghamshire; so in none of these districts is the series of coal-strata, as at present exposed, complete. The only place on the east, or rather the Midland district of the English coal-field where they have been is at Baxterley, near Nuneaton.

In the central and western coal-fields, they have been met with at Lane End in Staffordshire, Wellbatch, Leebotwood, Pontesbury, and Uffington, near Shrewsbury, Ardwick, near Manchester, and Whiston, near Liverpool; and in many of the Permian breccias and conglomerates of South Staffordshire and Shropshire they are to be found, proving their former occurrence in the districts

where such strata are now found. Probably the most complete series of these upper coal-measures is to be found at Ardwick, in the Manchester coal-field, where five different seams of coal and three beds of limestone have been profitably wrought. Here the deposit is rich in fossil organic remains, and contains a flora more distinct than any other in the British coal-fields, with the exception of that found low down in the Scottish series at Burdiehouse, near Edinburgh. That eminent palæontologist, Dr. Geinitz, of Dresden, after the examination of one of the best collections of these fossil plants, pronounced them to be true coal-measure specimens. The limestones were for a long period called freshwater; but after a careful examination of the mollusks, that well-known conchologist, Professor De Koninck, of Liége, came to the conclusion that they were all of an estuarine or marine character.

At Canobie, as previously stated, about 200 yards of upper coal-measures are met with above the Spirorbis limestone, and these are probably the highest carboniferous strata that have hitherto been discovered in England; and here also we have something like a passage of Carboniferous into Permian. I do not say, without making a cutting in the Duke of Buccleuch's land, just above the bridge, that I can absolutely prove that one formation passes into the other; but it is certainly the most likely place for it to be seen, if it really does occur in any place, and it has decidedly that appearance so far as it is at present exposed. The first opportunity that I have, it is my intention to obtain the consent of his Grace, and do all that I can to solve this interesting question of the passage of the carboniferous strata into the Permian, which has not yet been done in Great Britain.

At present, I have not been able to satisfy myself that the thick sandstone of Knotty Holm and Penton is of the

same age as the Whitehaven sandstone, although, if I had found large quartz-pebbles in it, I should have been inclined to do so from its other characters and fossil organic remains. If this sandstone can be proved to be the same, it will give us a much better idea as to where we are to place it, and show that it overlies the upper coal-measures with the Spirorbis limestone, and passes into red shales containing *Stigmaria,* until it goes into the soft red sandstone containing four beds of breccia. Professor Sedgwick, many years since, contended that the Whitehaven sandstone was the lowest member of the New Red Sandstone group, and says *, "After the first elevation of the central carboniferous chain of the north, the lowest division of the New Red Sandstone group (Rothtodt-liegende) was immediately deposited. The movements of elevation were not merely followed by, but were probably the mechanical causes of, this deposit, which is composed of sand, small pebbles, and other incoherent materials drifted to the outer edge of the coal-fields, even to this day in many places but imperfectly cemented, and contains, though rarely, a few drifted coal-plants. In some districts it is perfectly conformable to the upper coal-strata on which it immediately rests, and seems to form a regular connecting link between them and the overlying formations; but considered on the whole, its position, as far as regards the inferior strata, is discordant. It was followed and perhaps interrupted by movements of elevation producing a considerable disarrangement in its component beds, and of course also affecting the lower formations; and these movements were succeeded in several parts of Yorkshire and of the Cumbrian Mountains by deposits of magnesian conglomerate and magnesian limestone, unconformable to the

* "Introduction to the General Structure of the Cumberland Mountains," &c., by the Rev. A. Sedgwick, F.R.S., &c., Transactions of the Geological Society of London, (2nd series), vol. iv. p. 58.

lower division of the New Red Sandstone (Rothtodt-liegende) and to the coal-measures."

The passage of the Whitehaven sandstone into overlying strata cannot be seen, as it appears to have been much denuded prior to the deposition of the conglomerate on its eroded surface. At Astley and Bedford, as well as at Moira, we find no shaly beds above this sandstone; so, if the Knotty Holm sandstone should be proved to be of the same age as those strata, the beds lying between it and Canobie Bridge are deposits which have not yet been seen in any other section in Great Britain.

As previously stated in this communication, when the Whitehaven sandstone is present, a profitable coal-field is generally found near it. Now, in the Permian sections at Westhouse, as well as those of Kirkby Stephen, Belah, Brough, Hilton, and numerous ones about Dumfries and near Moffat in Scotland, the breccias or conglomerate-beds and the lower soft red sandstone are well exposed, but we see no trace of the Whitehaven sandstone under those beds; so, on the whole, that rock appears to be more nearly connected with the carboniferous than the Permian in these districts away from profitable coal-fields. The plants are generally in a fragmentary condition, and present the appearance of having been drifted; so it is possible they may have grown during the carboniferous epoch, and been drifted into the troubled waters in which the sandstone was formed. When, however, we observe it in Lancashire and Leicestershire, it is quite unconformable to the underlying coal-measures, and appears to have resulted from the movements of the earth's crust which followed on the elevation of the coal-measures, as supposed by Professor Sedgwick in the quotation previously given.

The Canobie section is the most complete series of Permian strata that has come under my notice in the north-

west of England or the south-west of Scotland. In it are shown the St. Bees sandstone, the red shales with gypsum, and a fine development of lower soft sandstone containing a singular bed of magnesian limestone imbedded in red shales and four beds of breccia, which at Barrowmouth is only seen in one bed of breccia 3 feet in thickness, although it is there accompanied by a bed of magnesian limestone found lying above it. This limestone has been as yet nowhere else met with in the border counties except at Canobie, and it is there found, as previously stated, in the middle of the red sandstone containing four beds of breccia. In addition to the Permian beds, we have probably the best-developed upper coal-measures, with a bed of Spirorbis limestone, yet seen in either England or Scotland.

In the Moat and Penton section only two beds of breccia were found in the soft sandstone; but the latter is not exposed, owing to some disturbance in the strata, as in the Canobie section; so they may be there without having been yet noticed. The passage of the Permian strata into the underlying or upper coal-measures above Canobie Junction is not so clearly seen as it is at Canobie Bridge; but the fault which brings in the millstone-grit series of coals against the upper coal-measures, called by Mr. Gibsone the Great Permian Fault, is well exposed in the railway-cutting at Penton.

In the Carlingway Burn section we have as yet found only one bed of breccia in the soft sandstones, and the latter only poorly developed when compared with those seen in the Canobie section; but the millstone grit and carboniferous limestones are well exposed, and their characters are so marked that there can be no question as to their geological age, and they appear to have been brought in by a great fault similar to that noticed in the Canobie and Penton sections.

The passage of the Moat (St. Bees) sandstone into the overlying soft sandstone (Trias) at Longtown is not well seen in any section which has come under my notice. This unfortunately is also the case with regard to the Dalston sandstone passing into the Holm Hill or Shawk sandstone. At Holm Hill as yet no traces of the red shaly clays on the underlying breccias and soft sandstone have been found, although the upper coal-measures of Raw Beck are well exposed to the extent of about 900 feet, and would seem to indicate the occurrence of a profitable coal-field in that neighbourhood, if it can be reached by sinkings of a moderate depth. Although we find the Spirorbis limestone present, we find none of the 200 yards of upper coal-measures seen at Canobie lying above it.

It has been shown that although the upper coal-measures of Lancashire and the Midland Counties of England contained several workable seams of coal, the same strata in Cumberland and Scotland contained none, notwithstanding that the strata were fully as largely or even more completely developed in the latter than the former. On the other hand, the mountain-limestone series in the latter districts contained numerous seams of coal, whilst none were to be found in the former.

The Canobie section affords us a singular example of the beds of breccia or conglomerate (No. 3 in the Table opposite) mingling with and dividing the lower soft red sandstone (No. 4) into several distinct beds, as well as giving us an instance of a bed of magnesian limestone being found in the midst of it. Some years since, I noticed the occurrence of two beds of conglomerate at Kirkby Stephen, and the intercalation of beds of breccia in the lower soft red sandstone of Ballochmoyle, the upper Rothliegende of the German geologists; but I never supposed that a bed of breccia sometimes formed its base, and three other distinct beds divided it. This example furnishes us

with a marked example of the variability of the Permian deposits of the north-west of England and the south-west of Scotland, and the difficulties in classifying them merely from their mechanical characters.

For the purpose of comparing the Permian beds at different places I give the following Table :—

TABULAR View of the Permian Strata of the North-west of England and the South-west of Scotland, as seen near Manchester, at Westhouse near Kirkby Lonsdale, Shawk near Carlisle, Barrowmouth near Whitehaven, and Moat near Longtown, in the descending order, and with approximate thicknesses *.

	Manchester.	Westhouse.	Shawk.	Barrowmouth.	Moat.
	feet.	feet.	feet.	feet.	feet.
1. Laminated and fine-grained red sandstones (St. Bees)	Not seen.	Not seen.	300	1000	30
2. Red and variegated clays or marls containing sometimes, but not always, beds of limestone and gypsum, and bands of sandstone, the clays and limestones containing fossil shells of the genera *Schizodus*, *Bakevellia*, &c.	300	Traces of them seen.	150	70	225
3. Conglomerate or breccia.	50	300	4	3	600
4. Lower Red Sandstone, generally soft and incoherent.	500	500	7	Not seen	
5. Red shaly clays.	Not seen	250	Not seen	Not seen	Not seen
6. Astley pebble-beds containing common coal-plants, but quite unconformable to the upper coal-measures, termed by me Lower Permian.	60	Not seen	Not seen	140	Not seen

* The first four strata of the above series, Professor Harkness, F.R.S., in a fine natural section seen at Hilton Beck, near Brough, estimates to be fully 3000 feet in thickness. He also states that he has found the marl-slate there, with its characteristic fossils. There are certainly some cream-coloured beds seen in that locality, which contain fragments of plants; but they are in such a bad state of preservation that I have been unable to decipher them, except that one plant is a *Pecopteris*. My collection is a pretty good one, and collected by myself.

With respect to the Triassic beds, so far as yet examined in the district comprised in this paper, they appear to differ from the typical beds seen in Cheshire and South Lancashire as to their divisions where Mr. Hull divides the Trias as follows * :—

Formation.	Subformation.	Thickness.
		feet.
KEUPER..	Red marl	3000
	Lower Keuper sandstone	450
BUNTER..	Upper mottled sandstone	700
	Pebble-beds	800
	Lower mottled sandstone	800
		5750

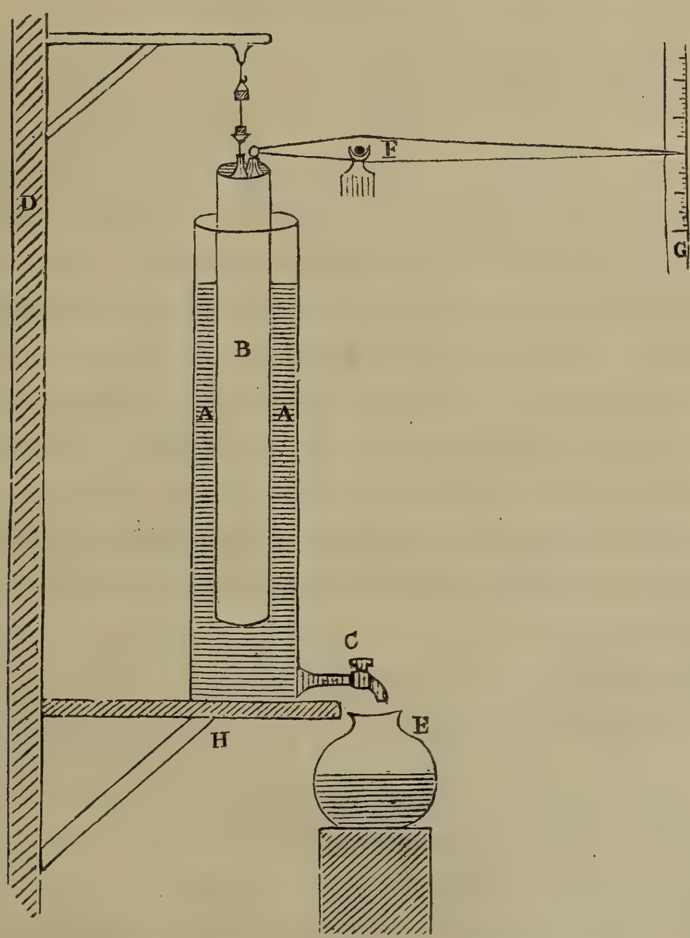
In the soft, red, variegated and false-bedded sandstones of Longtown, Rockcliffe, Holmhead, and Dalston, we have a rock which would correspond with either the lower or the upper mottled sandstone of that author; but as yet I have seen nothing of the pebble-beds. The whole of the Triassic beds appear to have been deposited under circumstances of great quietude, very different to those in which the Permian beds were formed. The two divisions of the Keuper are well developed in the waterstones and red and variegated marls containing beds of gypsum, but, so far as yet known, without affording beds of salt, although it is probable they may be there. The red marls no doubt pass upwards into the Lias at Quarry Gill and Oughterby, but I have seen no natural section showing the passage of the one into the other.

* "On the New Subdivisions of the Triassic Rocks of the Central Counties," by E. Hull, A.B., F.G.S., Transactions of the Manchester Geological Society, vol. ii. p. 31.

XXVIII.—*On an Apparatus for Measuring Tensile Strengths, especially of Fibres.* By CHARLES O'NEILL, Esq., F.C.S.

Read November 17th, 1863.

IN the sketch of the apparatus, A is a tin vessel to hold water, provided with a cock, C. B is a hollow cylinder of glass or metal, closed permanently at the lower end, and so weighted as to float vertically in stable



equilibrium with about one-eighth of its length out of water; at the upper end, a hook or other contrivance is

fixed for holding the substance to be tested. D is a fixed support, provided with means of attaching the fibre to be tested. F is a lever with a long and short arm; and G is a graduated scale, over which the point of the long arm of the lever moves. H is a table or support upon which the apparatus rests; and E is a measuring-flask or bottle which serves to ascertain the amount of water drawn off from A.

To use the apparatus, it is nearly filled with water, and the fibre, thread, or wire to be tested is secured to the fastenings on B and D in a slack state; water is drawn off from the cock C until the fibre, &c., is taut, or until the long arm of the lever ceases to move over the scale. The measuring- or weighing-flask is then put under the cock, and the water allowed to flow into it in a regular stream, with the hand upon the tap, until the fibre breaks, when the cock is instantly turned; the quantity of water is then ascertained, and from it the amount of strain upon the fibre or thread at the moment of rupture. The movements of the long arm of the lever are watched during the operation, and noted at the moment of breaking; this gives the stretch of the fibre or thread, and serves to correct the indications given by the water-flask. To prevent damage to the apparatus by the falling of the cylinder upon the breaking of the thread, a stop, not represented in the sketch, is fixed, which only allows a short fall of half an inch or so.

The principle of the apparatus is so simple, that it hardly requires any explanation. At the beginning of the operation the tube is supported by the water, and there is no weight upon the fibre; by drawing off the water the support is gently removed, and the weight of the cylinder gradually thrown upon the fibre or thread.

The weight thrown upon the fibre is in relation to the weight or volume of water drawn off; but the ratio between these quantities will vary for every different dimen-

sion of the floating cylinder and the containing vessel, but it will always be in the direct ratio of the sectional areas of the floating cylinder and the ring of water surrounding it.

The apparatus I have actually used in my experiments upon textile fibres consists of an outer vessel provided with a tap, made of common tin plate, standing 13 inches high and about $3\frac{1}{2}$ inches in diameter, possessing a sectional area of about 9·8 inches. I have used three floating cylinders: the larger one, made of tin plate, has a circumference of 8 inches, and a sectional area of about 5·09 inches; the medium cylinder is of glass, having a circumference of 2·4 inches, a sectional area of 0·458 inch; and the smaller one is a fine-stemmed glass hydrometer, having a circumference of 0·5 inch, and a sectional area of ·001989 inch.

The ratios of the areas of these floating cylinders to the difference between them and that of the fixed cylinder (being the area of the ring of water) are as follows:—

For the larger	1	:	0·925
For the medium	1	:	20·61
For the smaller	1	:	492·6

Therefore, supposing these measurements correct, and the fixed and floating cylinders perfectly cylindrical and even throughout, 1 grain weight would be put upon the fibre by the withdrawal of 0·925 grain of water for the larger, 20·61 grains for the medium, and 492·6 grains for the smaller floating cylinder.

But, actually, neither the fixed nor floating cylinders in my apparatus are so accurately made as to justify a reliance upon arithmetical results; and I have graduated them by actual testing with a delicate chemical balance, ascertaining the weight required to keep the floating cylinders at one constant level for various quantities of water

drawn off: the results came out as follows, the temperature of the water being 56° F.:—

Larger cylinder: 1000 grains of water drawn off, at 5 different levels, equal 1080 grs. weight; or 0·926 gr. water equal 1 grain weight.

Medium cylinder: 1000 grs. water drawn off, at 7 different levels, equal a mean of 47·4 grs.; or 21·09 gr. water drawn off equal 1 grain weight.

Smaller cylinder: 10,000 grains water drawn off equal 21 grs.; or 476·1 grs. water equal 1 gr. weight.

These practical data are as near the calculation as I expected them to come, and I have used them in my experiments.

In the case of the fibre or thread stretching before breaking, a correction will have to be made for the extent of stretch which lowers the floating cylinder in the water; to make this correction it will be necessary to determine experimentally for each cylinder how much water must be drawn off to lower it a given distance when it is free to move. This is very easily done, by means of an index and a graduated scale. The medium cylinder, for example, is lowered 0·1 inch by drawing off 250 grains of water: it is evident if a fibre of cotton had stretched a tenth of an inch before breaking, the whole water drawn off would be 250 grains more than if it had remained rigid; this quantity must therefore be deducted from the quantity drawn off, in calculating the breaking-weight. The temperature of the water will also influence the results slightly; but I have always used water between 50° and 60° Fahr., and the correction is too slight to be required for the purposes to which I have yet applied the apparatus.

As an example of the method of using the apparatus, I give the results of an experiment upon Sea Island cotton, made with the medium glass cylinder. A single fibre was taken by a forceps, and, by means of a camel's-hair pencil,

secured at each end between gummed paper, which was fastened to fine wire triangles; a triangle was hung upon the fixed support, and the other brought on to the hook of the floating cylinder by means of an adjustment on the fixed support (not shown in the sketch); the fibre was drawn until nearly tight; water was then slowly drawn off until the fibre was observed to be quite straight and taut; then the lever was set and noted, and water slowly run off into a measuring-vessel until the fibre broke. The long arm of the lever had moved over $1\frac{1}{2}$ division of the scale, $=0{\cdot}15$ inch; but, as the lever multiplies six times, the actual stretch or depression of the cylinder before breaking was only 0·025 inch. The quantity of water drawn off was found to be 3300 grains, from which must be deducted 62 grains for the stretch, leaving 3238 grains as the breaking-quantity; this, being divided by 21·09, gives 153·5 grs. as the actual breaking-weight for this fibre.

It will be observed that this apparatus has two special features—namely, the very gradual manner in which the tension can be applied without jerks or shocks, and the extreme sensibility of which it is capable. The smaller floating cylinder can easily put on a measured strain of 0·002 grain; and with larger water-vessels or smaller floating cylinders there is hardly any assignable limit to its delicacy. Of course great care and numerous precautions are necessary when measuring strains so small.

On the other hand, the apparatus may be enlarged to a size that could put on a strain of a hundred tons, and could perhaps be applied to many of the cases of testing wires, chains, or castings.

I believe that this apparatus may be advantageously employed in many experimental researches, where it is desired to exercise a measured and constant strain. Hitherto I have only employed it in ascertaining the elasticity and tensile strength of textile fabrics, for which it seems spe-

cially adapted; the only precaution necessary is to keep the floating cylinder from contact with the sides of the containing vessel, which is easily done by means of guides, either fixed on the cylinder or acting from above.

XXIX.—*Experiments and Observations upon Cotton.*

By Charles O'Neill, Esq., F.C.S.

Read November 17th, 1863.

Some years ago I commenced a research into the chemical principles of cotton-dyeing, with a view to elucidate several disputed questions concerning the bearings of dyeing-phenomena upon chemical laws in general. Like most other experimenters in this field, I began upon cotton cloth, but soon found no scientific data were to be obtained from the fibre in the woven state, since only those fibres and parts of fibres which were exposed on one side or other of the cloth came into contact with the reagents, the great bulk not being at all acted upon. In yarn this objection was only lessened, not removed; and when at length I found in many instances that a single fibre of cotton, less than $\frac{1}{800}$ of an inch in diameter, might be dyed upon one of its flattened sides and quite colourless on the other, it became clear that experiments should be made upon the primary fibre. In consequence my attention was turned to it; and although the main object of my experiments has not been attained, I have collected a number of observations which, having been made with great deliberation and exactness, I wish to communicate and put on record concerning the fibre.

The term "cotton fibre" has a kind of collective signification, meaning a mass of cotton fibres, and, if used for the

individual fibres, might lead to want of clearness; I propose therefore to employ the word *hair*, or the term *cotton hairs*, when referring to the fibres singly.

Experimenters appear to have been deterred from manipulating with the individual hairs, on account of their smallness and lightness; and when we consider that there are from fourteen to twenty thousand hairs in a single grain-weight of cotton, and that they are so small as $\frac{1}{2000}$th of an inch in diameter, we might consider them exclusively within the domain of the microscopist; and not a little of the interest I have felt in these experiments has been derived from the reflection that I was ascertaining the properties of perhaps the smallest particles of matter which had been subjected to similar experimental research.

I propose in this paper to confine myself to some of the physical properties of the hairs of several varieties of cotton as found in commerce, and hope in future communications to arrange some more miscellaneous matter.

The Samples of Cotton experimented upon.—The experiments have been made upon seventeen different samples of cotton. Eight of these samples I obtained through the Cotton Supply Association about three years ago, in answer to my request; and I am indebted to Mr. G. R. Haywood, then acting as secretary to the Association, for his readiness in forwarding them to me. There can be no doubt that these samples properly represent their various denominations: their names and prices in Liverpool, on December 13, 1860, are given below:—

	d.	
Surat (Dhallerah)	$5\frac{1}{4}$	per lb.
Mobile	$6\frac{7}{8}$	,,
Upland	$6\frac{7}{8}$	,,
Orleans	$7\frac{1}{8}$	,,
Maranham	$8\frac{1}{4}$	,,
Egyptian	$9\frac{1}{4}$*d.* to $9\frac{1}{4}$	,,
Sea Island	16	,,
Sea Island, grown at Edisto Island	26	,,

Eight other samples were most kindly obtained for me by the late G. Mosley, Esq., who was much interested in my experiments: in a note accompanying them, he says, "Their origin may be depended upon; they are direct from the first-hand brokers to my friend, who is a selling broker." They are further marked with the names of the vessels in which they were imported, and are, I believe, entitled to equal confidence with the preceding eight samples. The following is a list of Mr. Mosley's samples:—

	March 28, 1863.
Sea Island, good quality	54*d.* per lb.
Pernambuco	23 "
Maranham, good middling	...
Egyptian, fair	22 "
Benguela (Portuguese Africa)	...
New Orleans, good middling	22½ "
Surat Dhallera, "fair, of very good character"	17¾ "
Surat Comptah, middling	15 "

I am indebted to W. H. Heys, Esq., for a small sample of Queensland cotton, making seventeen samples in all—of which twelve are of different origin, and five duplicates but probably grown at different times or under different circumstances.

Length of the Hairs.—One of the distinguishing marks of various samples of cotton is the different lengths of the hairs or staple. I have determined the lengths of at least twenty hairs of each of the samples of cotton, and have given the particulars of each measurement; but, as I believe these are the first measurements of the fibre ever made upon the individual fibres, it is necessary to allude to the method employed. Many writers have given lengths of the fibres of various qualities of cotton; and the method generally employed is to draw out a tuft of the cotton repeatedly between the fingers until the hairs are laid parallel, or nearly so, and then to measure the length of the tuft. This is, I believe, the mercantile test for the length

of staple, and it is a satisfactory one; but it is very evident that it has no pretensions to be exact, and can only give a rough average of the length of the hairs. The latest reports of experiments upon cotton that I have seen are by Captain J. Mitchell, of the Government Central Museum in Madras, dated the 21st and 25th April and 10th June 1862; they are addressed to the Madras Government, and published and circulated by the order of that Government. In this report, Captain Mitchell gives a large number of micrometer-measurements of the breadth of the hairs in eighteen samples of cotton; I think these measurements leave nothing to desire, and I have pleasure in drawing the attention of the Society to them. But Captain Mitchell desired to extend his researches to the determing the length of the hairs; and, speaking upon this point, he writes, "It is with much regret that I am obliged to say that I have not been able to devise any easy and practical method of measuring single fibres. The exceeding tenuity of the fibre (say, as a mean, $\frac{1}{1200}$th of an inch in diameter) renders it impossible to apply it to an ordinary scale; and the only method I can see is to cement them to a glass slide, which can then be applied to a scale ruled on glass for that purpose. This is exceedingly tedious, and very trying to both eyes and head, from the impossibility of seeing the fibres without a lens; I therefore abandoned it for the method adopted (I believe) by the broker and manufacturer, of repeatedly drawing a small portion through the fingers until it appears to be nearly all in a line, and then measuring the small tufts." And in another part of the report the writer gives figures and diagrams of the measurements so taken. Difficulties disappear by labour and application. I am now able to measure individual fibres by a simple and exact method, and with tolerable rapidity. The necessary appliances consist of a piece of window glass, in the centre of which

a scale of 2 inches, divided into tenths, is laid down with a diamond, a pair of forceps to lay hold of the hairs, and two camel's-hair pencils moderately stiff; and the method of working is as follows:—I lay out the sample of cotton I am about to try, and take a pinch from half a dozen different parts, mix them together by pulling between the fingers, and then roll into a loose ball. I lay this cotton upon a piece of black cloth, and the glass scale is also laid upon black cloth; then with the forceps I take up the first fibre I put my eye on, and lay it on the glass plate. Every ordinary fibre of cotton is thick at one end, and thin and tapering at the other; and as soon as I see which is the thicker end, I lay hold of that part, but not quite at the end, with the forceps in my right hand, and with a moist camel's-hair pencil in my left hand, press gently upon the fibre and with the forceps draw it between the pencil and the glass until the extremity is over the first mark of the scale; I then roll the pencil over a little towards the thicker end of the fibre, so as to take a good of hold it, drop the forceps, and with the other camel's-hair pencil stroke the fibre down on the scale, and read off its length to the twentieth of an inch. I go all round the ball of cotton, not choosing the hairs, but taking the first that offers to the eye or hand: if the hair be evidently a broken one, and has not the tapering extremity, I reject it; but there are very few of these in unmanufactured cotton; it is usually the other end which is broken. Cotton fibres are in a crumpled state, and require some force to straighten them out, the elasticity of good cotton being considerable; but the glutinous matter from the saliva with which the pencils are wetted will hold them down on the glass pretty well: but generally it is necessary to read off instantly the measure of the fibre; for the ends will curl up, if left, and the fibre most likely be lost. I do not mean to say that I arrived at this simple method all at once, or, even now, that it does

not require some manipulative dexterity; but it is quite practicable, and without any lens, and, simply by arranging the light properly, it is possible to measure cotton hairs accurately and quickly.

In the measurements given in the following tables, I have never pretended to measure closer than $\frac{1}{20}$th of an inch (my scale only indicates tenths); and when the extremity of the hair lay between one division and the other, I judged by the eye whether it was less or more than one-fourth or three-fourths of a division, and read it out accordingly. Upon the diagram, I have laid down upon an enlarged scale of ten to one the results of the measurements, showing the mean lengths of all the hairs, and the lengths of the longest and shortest of the hairs measured.

Length of 20 fibres of Sea Island cotton grown at Edisto. Sample furnished by Cotton Supply Association, December 13, 1860; price then 2*s*. 2*d*. per lb.

No.	in.	No.	in.	No.	in.	No.	in.
5 ...	2·0	4 ...	1·75	13 ...	1·70	17 ...	1·55
9 ...	1·95	8 ...	1·75	15 ...	1·70	12 ...	1·55
1 ...	1·80	10 ...	1·75	16 ...	1·70	11 ...	1·45
2 ...	1·80	3 ...	1·70	7 ...	1·60	18 ...	1·45
20 ...	1·80	6 ...	1·70	14 ...	1·55	19 ...	1·35

Mean length = 1·68 inch.
Longest fibre = 0·32 inch longer than mean.
Shortest fibre = 0·33 inch shorter than mean.

This cotton possesses the greatest mean length of all the samples measured; the shortest and longest hairs are nearly equally distant from the mean, and 25 per cent. of the hairs have nearly the mean length. In the case of one other sample only is the length of the longest hair exceeded, while its shortest is considerably above that of any other sample. A very fine and exceptional cotton.

Length of 32 fibres of Sea Island cotton, marked "good quality," March 31, 1863, 54*d.* per lb. Sample supplied by G. Mosley, Esq.

No.	in.	No.	in.	No.	in.	No.	in.
6	1·95	10	1·65	25	1·50	28	1·40
7	1·75	2	1·60	30	1·50	31	1·40
20	1·75	12	1·60	32	1·50	5	1·35
1	1·70	9	1·55	19	1·45	14	1·30
3	1·70	11	1·55	22	1·40	26	1·30
4	1·70	17	1·50	15	1·40	18	1·15
8	1·70	21	1·50	23	1·40	13	1·10
29	1·70	24	1·50	27	1·40	16	1·10

Mean length = 1·501 inch.
Longest fibre = 0·449 inch longer than mean.
Shortest fibre = 0·401 inch shorter than mean.

This is the second cotton in mean length, and shows a considerable range between the longest and shortest hairs; more than half the hairs are, however, of or above the mean length; a few very short ones bring down the average.

Length of 20 fibres of Queensland cotton. From a small sample supplied by W. H. Heys, Esq.

No.	in.	No.	in.	No.	in.	No.	in.
20	1·80	13	1·60	5	1·45	1	1·30
19	1·70	16	1·60	12	1·45	4	1·30
15	1·65	6	1·50	2	1·40	11	1·30
9	1·60	8	1·50	14	1·40	7	1·25
10	1·60	17	1·50	18	1·40	3	1·20

Mean length = 1·475 inch.
Longest fibre = 0·325 inch longer than mean.
Shortest fibre = 0·255 inch shorter than mean.

This cotton, which can hardly be said to be in the market, yet approaches very nearly to Sea Island at 54*d.*; it has fewer of the long hairs of 1·7 or 1·75, but is compensated by having none so low as 1·1, and only 10 per cent. below 1·3 inch. There is also considerably less range in the lengths.

Length of 33 fibres of Sea Island cotton. Sample from Cotton Supply Association. Price 16*d.* on December 13, 1860.

No.	in.	No.	in.	No.	in.	No.	in.
6 ...	2·05	10 ...	1·55	26 ...	1·40	20 ...	1·30
16 ...	1·90	25 ...	1·55	23 ...	1·35	30 ...	1·30
8 ...	1·70	27 ...	1·55	31 ...	1·35	24 ...	1·25
29 ...	1·70	13 ...	1·50	2 ...	1·30	11 ...	1·20
9 ...	1·65	28 ...	1·50	4 ...	1·30	32 ...	1·20
7 ...	1·65	18 ...	1·45	12 ...	1·30	14 ...	1·15
15 ...	1·65	1 ...	1·40	17 ...	1·30	3 ...	1·10
22 ...	1·65	5 ...	1·40	19 ...	1·30	33 ...	1·10
21 ...	1·60						

Mean length = 1·444 inch.
Longest fibre = 0·606 inch greater than mean.
Shortest fibre = 0·344 inch less than mean.

This cotton presents the longest hair I have measured, and shows a difference of nearly 1 inch between the longest and shortest hairs. This is a considerable range; but the average stands high.

These four examples of cotton, viz. three Sea Island and one Queensland, may be called our long-staple cottons. Egyptian is, I believe, also reckoned a long staple; but between it and the shortest Sea Island there is a wider gap than occurs between any two succeeding qualities taken in the order of their lengths; we may therefore consider them as a group. The Edisto Island cotton, however, stands out alone; being 0·18 inch longer in the mean than the next highest Sea Island, it may be taken as exceptionally long. The other three samples are very nearly of the same length in the mean, Queensland standing between two different samples of Sea Island, being 0·026 inch shorter than one, and 0·031 inch longer than the other: the longest hair of the Queensland is from 0·25 to 0·15 shorter than the longest hairs of the Sea Island; but, on the other hand, its shortest hairs are 0·1 inch longer than the shortest hairs of the Sea Island.

Length of 20 hairs of Egyptian cotton. Furnished by the Cotton Supply Association, December 13, 1863. Price, at above date, 9¼*d.* to 9½*d.*

No.	in.	No.	in.	No.	in.	No.	in.
2	1·55	7	1·35	5	1·20	6	1·10
18	1·50	8	1·30	9	1·20	20	1·10
3	1·45	19	1·30	11	1·20	10	1·05
4	1·45	1	1·25	13	1·20	17	1·05
12	1·40	15	1·25	16	1·20	14	0·95

Mean length = 1·252 inch.
Longest fibre = 0·298 inch longer than mean.
Shortest fibre = 0·302 inch shorter than mean.

In this cotton, which stands next to Sea Island for mean length, we find a difference in the mean of 0·192 inch, while in the longest hairs of each we have a difference of 0·5 inch, and in the shortest a difference of 0·15. It is a tolerable even cotton, the extremes not being very far apart, and but few hairs presenting extreme measurements, not less than one-half of the whole number of hairs being within 0·1 inch of the mean. The next two samples of cotton approach very near to this, the Maranham being within 0·032 inch of the mean length, and the second sample of Egyptian within 0·035 inch of the Maranham. This sample of Maranham has no hairs so long as the longest hairs of the Egyptian; but it has many hairs of 1·4 and 1·3, which bring up the average to nearly the same, while its shortest hairs are the same as the Egyptian; it may be considered very even, so great a number of hairs being close together. The second sample of Egyptian falls 0·067 inch below the first, owing to a want of long hairs; it is much less even than the Maranham, the extremes being 0·65 apart, while in the Maranham they are only 0·45 apart.

Length of 20 hairs of Maranham cotton. Sample supplied by G. Mosley, Esq., March 31, 1863. No price.

No.	in.	No.	in.	No.	in.	No.	in.
1 ...	1·4	3 ...	1·30	4 ...	1·20	13 ...	1·10
12 ...	1·4	6 ...	1·30	9 ...	1·20	19 ...	1·10
15 ...	1·4	7 ...	1·30	2 ...	1·15	10 ...	1·05
14 ...	1·35	16 ...	1·30	5 ...	1·10	11 ...	1·05
18 ...	1·35	17 ...	1·30	8 ...	1·10	20 ...	0·95

Mean length = 1·22 inch.
Longest fibre = 0·18 inch longer than mean.
Shortest fibre = 0·27 inch shorter than mean.

Length of 20 hairs of "Fair Egyptian" cotton. Sample supplied by G. Mosley, Esq., March 28, 1863. Price 22*d.*

No.	in.	No.	in.	No.	in.	No.	in.
8 ...	1·50	15 ...	1·30	2 ...	1·15	9 ...	1·05
11 ...	1·40	16 ...	1·30	10 ...	1·15	19 ...	1·05
12 ...	1·40	3 ...	1·20	7 ...	1·10	14 ...	1·00
20 ...	1·40	4 ...	1·20	13 ...	1·10	5 ...	0·95
1 ...	1·30	17 ...	1·20	18 ...	1·10	6 ...	0·85

Mean length = 1·185 inch.
Longest fibre = 0·315 inch longer than mean.
Shortest fibre = 0·335 inch shorter than mean.

Length of 20 hairs of Benguela (Portuguese African) cotton. Supplied by G. Mosley, Esq., March 31, 1863. No quotation.

No.	in.	No.	in.	No.	in.	No.	in.
9 ...	1·5	14 ...	1·30	7 ...	1·15	18 ...	1·10
2 ...	1·4	3 ...	1·25	15 ...	1·15	20 ...	1·05
8 ...	1·35	10 ...	1·25	19 ...	1·15	11 ...	0·95
1 ...	1·30	4 ...	1·20	6 ...	1·10	17 ...	0·90
5 ...	1·30	12 ...	1·20	13 ...	1·10	16 ...	0·85

Mean length = 1·177 inch.
Longest fibre = 0·373 inch longer than mean.
Shortest fibre = 0·327 inch shorter than mean.

Length of 20 of hairs Pernambuco cotton. Sample supplied by G. Mosley, Esq., March 28, 1863. Price 23*d.* per lb.

No.	in.	No.	in.	No.	in.	No.	in.
16	... 1·50	1	... 1·30	18	... 1·15	12	... 1·05
19	... 1·45	9	... 1·30	20	... 1·15	14	... 1·05
13	... 1·40	7	... 1·20	6	... 1·10	2	... 0·90
15	... 1·40	10	... 1·15	3	... 1·05	11	... 0·90
8	... 1·35	17	... 1·15	4	... 1·05	5	... 0·75

Mean length = 1·1675 inch.
Longest fibre = 0·3325 inch longer than mean.
Shortest fibre = 0·4125 inch shorter than mean.

The samples of Benguela and Pernambuco are nearly equal in lengths and in differences, and approach very closely to the preceding samples of Egyptian and Maranham; but both show a wide range between the longest and shortest hairs.

Length of 20 hairs of Maranham cotton. Sample furnished by the Cotton Supply Association, December 13, 1860. Price 8½*d.*

No.	in.	No.	in.	No.	in.	No.	in.
16	... 1·35	9	... 1·25	20	... 1·15	3	... 0·95
8	... 1·30	4	... 1·20	7	... 1·10	5	... 0·95
11	... 1·30	10	... 1·20	17	... 1·10	12	... 0·95
15	... 1·30	6	... 1·15	19	... 1·10	2	... 0·90
18	... 1·30	14	... 1·15	13	... 1·00	1	... 0·85

Mean length = 1·127 inch.
Longest fibre = 0·223 inch longer than mean.
Shortest fibre = 0·227 inch shorter than mean.

This sample of Maranham is rather considerably different from the previous sample of the same name: it is nearly 0·1 shorter in the mean (which is owing to the absence of long hairs of 1·4), and a shorter staple throughout; it is not so even either, but there is not much difference in that respect.

The next four samples of cotton are from the Southern States of North America, and present very small differences.

In the mean lengths Mobile is the longest, and one sample of "good middling" Orleans the shortest; but the difference is only 0·065. The longest and shortest hairs are found occurring in the sample of Orleans; but the greatest differences between the samples in this respect amount only to 0·10 for the longest, and 0·15 for the shortest hairs. These four samples may be considered as forming a group by themselves, but not a very distinct group, since they merge as easily into East Indian cotton as into one another; but still they are separate.

Length of 20 hairs of Mobile cotton. Sample furnished by the Cotton Supply Association. Price, December 13th, 1860, $6\frac{7}{8}d$.

No.	in.	No.	in.	No.	in.	No.	in.
13	... 1·20	19	... 1·15	6	... 1·05	18	... 1·00
3	... 1·15	8	... 1·10	5	... 1·00	4	... 0·90
9	... 1·15	14	... 1·10	7	... 1·00	2	... 0·85
12	... 1·15	20	... 1·10	11	... 1·00	10	... 0·85
17	... 1·15	1	... 1·05	16	... 1·00	15	... 0·75

Mean length = 1·035 inch.
Longest fibre = 0·165 inch longer than mean.
Shortest fibre = 0·285 inch shorter than mean.

For the mean lengths, the difference of the longest and shortest hairs is considerable; but not less than 75 per cent. are within 0·1 of the mean, which shows great regularity.

Length of 20 hairs of Orleans cotton. Sample furnished by the Cotton Supply Association, December 13th, 1860. Price $7\frac{1}{8}d$.

No.	in.	No.	in.	No.	in.	No.	in.
15	... 1·25	5	... 1·15	18	... 0·95	17	... 0·90
1	... 1·20	8	... 1·15	19	... 0·95	7	... 0·85
12	... 1·20	4	... 1·05	6	... 0·90	9	... 0·80
16	... 1·20	11	... 1·05	10	... 0·90	14	... 0·75
20	... 1·20	3	... 1·00	13	... 0·90	2	... 0·70

Mean length = 1·002 inch.
Longest fibre = 0·248 inch longer than mean.
Shortest fibre = 0·302 inch shorter than mean.

Not much difference in the mean results from the sample of Mobile; but a greater number of hairs are more distant from the mean, less than 50 per cent. coming within 0·1 of the mean.

Length of 20 hairs of Upland cotton. Sample furnished by Cotton Supply Association, December 13th, 1860. Price $6\frac{7}{8}d$.

No.	in.	No.	in.	No.	in.	No.	in.
17 ...	1·20	12 ...	1·05	1 ...	0·95	14 ...	0·90
18 ...	1·20	20 ...	1·05	6 ...	0·95	19 ...	0·90
7 ...	1·15	4 ...	1·00	8 ...	0·95	3 ...	0·85
15 ...	1·15	9 ...	1·00	16 ...	0·95	5 ...	0·80
13 ...	1·10	10 ...	1·00	2 ...	0·90	11 ...	0·80

Mean length = 0·9925 inch.
Longest fibre = 0·2075 inch longer than mean.
Shortest fibre = 0·1925 inch shorter than mean.

The lengths here are more regular and within a narrower compass than in the preceding sample, while the mean length is but slightly different; it is therefore more even and regular in length of staple.

Length of 20 hairs of "good middling Orleans" cotton. Sample supplied by G. Mosley, Esq., March 28, 1863. Price $22\frac{1}{2}d$.

No.	in.	No.	in.	No.	in.	No.	in.
3 ...	1·15	10 ...	1·00	7 ...	0·95	11 ...	0·90
13 ...	1·15	14 ...	1·00	8 ...	0·95	17 ...	0·90
2 ...	1·05	20 ...	1·00	12 ...	0·95	19 ...	0·90
9 ...	1·05	1 ...	0·95	4 ...	0·90	16 ...	0·85
15 ...	1·05	6 ...	0·95	5 ...	0·90	18 ...	0·85

Mean length = 0·970 inch.
Longest fibre = 0·180 inch longer than mean.
Shortest fibre = 0·120 inch shorter than mean.

Though these hairs show a less mean than the preceding two samples, they lie well together, 80 per cent. being within 0·1 inch of the mean, and the extreme difference being 0·3 inch.

The next three samples are of East Indian cotton, and present a sensibly less mean length than the North American cotton, but not to a greater extent than exists amongst the American cottons themselves; and the means are only brought down by there being fewer of the longer hairs, which, however, are not quite so long as in the American cottons; the short hairs are few, and only a little shorter than the short ones in American.

Length of 20 hairs of Surat cotton. Sample from G. Mosley, Esq., labelled "Fair Dhallerah, of very good character." March 31, 1863. Price 17¼*d.*

No.	in.	No.	in.	No.	in.	No.	in.
20 ..	1·15	7 ...	1·00	14 ...	0·95	4 ...	0·85
1 ...	1·10	9 ...	1·00	12 ...	0·90	8 ...	0·85
5 ...	1·05	15 ...	1·00	16 ...	0·90	13 ...	0·85
19 ...	1·05	3 ...	0·95	17 ...	0·90	2 ...	0·75
6 ...	1·00	10 ...	0·95	18 ...	0·90	11 ...	0·75

Mean length = 0·9425 inch.
Longest fibre = 0·2075 inch longer than mean.
Shortest fibre = 0·1925 inch shorter than mean.

The lengths of these hairs are very close about the mean; it may be said that 80 per cent. are within 0·1 of the mean, by counting the two lengths 1·05 as being within.

Length of 20 hairs of Surat (Dhallerah) cotton. Sample from Cotton Supply Association, December 13, 1860. Price then, 5¼*d.* per lb.

No.	in.	No.	in.	No.	in.	No.	in.
3 ...	1·10	16 ...	1·00	15 ...	0·90	19 ...	0·85
4 ...	1·10	5 ...	0·90	17 ...	0·90	20 ...	0·80
12 ...	1·10	6 ...	0·90	18 ...	0·90	8 ...	0·75
7 ...	1·05	9 ...	0·90	1 ...	0·85	14 ...	0·75
13 ...	1·00	10 ...	0·90	11 ...	0·85	2 ...	0·55

Mean length = 0·925 inch.
Longest fibre = 0·185 inch longer than mean.
Shortest fibre = 0·375 inch shorter than mean.

This sample is 0·0175 below the preceding, and in it occurs the shortest hair of which measurement is recorded; but, without that, the run of the hairs is less than in the other sample of Dhallerah all through.

Length of 20 hairs of Surat (Comptah, middling) cotton. Sample supplied by G. Mosley, Esq., March 28, 1863. Price 15*d.*

No.	in.	No.	in.	No.	in.	No.	in.
4	... 1·05	6	... 0·95	5	... 0·90	9	... 0·85
18	... 1·05	8	... 0·95	7	... 0·90	14	... 0·80
20	... 1·05	12	... 0·95	10	... 0·90	11	... 0·75
16	... 1·00	13	... 0·95	15	... 0·90	1	... 0·70
19	... 1·00	2	... 0·90	3	... 0·85	17	... 0·70

Mean length = 0·905 inch.
Longest fibre = 0·145 inch longer than mean.
Shortest fibre = 0·205 inch shorter than mean.

This is decidedly in every measurement the shortest staple cotton of those examined, but pretty regular in the lengths of the hairs, and for the lengths not a great difference in the extremes.

In the following Table I have brought together the lengths of the longest hairs in each sample, the mean length, and the length of the shortest hair, and have arranged them in the order of the greatest mean lengths.

TABLE showing the lengths of the longest and shortest hairs, and mean length of 20 hairs, from various samples of cotton.

			Longest fibre.	Mean length.	Shortest fibre.
	d.		in.	in.	in.
Sea Island, Edisto	26	Dec. 1860	2·00	1·680	1·35
Sea Island	54	Mar. 1863	1·95	1·501	1·10
Queensland cotton	......	....	1·80	1·475	1·20
Sea Island "	16	Dec. 1860	2·05	1·444	1·10
Egyptian "	$9\frac{1}{4}$ to $9\frac{1}{2}$	Dec. 1860	1·55	1·252	0·95
Maranham "	......	....	1·400	1·220	0·95
Egyptian fair "	22	Mar. 1863	1·500	1·185	0·85
Benguela "	......	....	1·500	1·177	0·85
Pernambuco "	23	Mar. 1863	1·500	1·1675	0·75
Maranham "	$8\frac{1}{2}$	Dec. 1860	1·35	1·127	0·85
Mobile "	$6\frac{7}{8}$	Dec. 1860	1·20	1·035	0·75
Orleans "	$7\frac{1}{8}$	Dec. 1860	1·25	1·002	0·70
Upland "	$6\frac{7}{8}$	Dec. 1860	1·20	0·9925	0·80
Orleans (good midd.)...	$22\frac{1}{2}$	Mar. 1863	1·15	0·970	0·85
Surat (fair Dhallerah)...	$17\frac{3}{4}$	Mar. 1863	1·15	0·9425	0·75
Surat (Dhallerah)	$5\frac{1}{4}$	Dec. 1860	1·10	0·925	0·55
Surat (middl. Comptah)	15	Mar. 1863	1·05	0·905	0·70

Strength of Cotton Hairs.—If experimenters have recoiled before the supposed difficulties of measuring the individual hairs of cotton, so much the more have they avoided even the mention of testing their tensile strengths; but, by the aid of the little apparatus described in a previous paper, I have made, in the course of my investigations, many hundred determinations of the tensile strength of cotton hairs, and, taking into consideration the nature of the fibre, with quite satisfactory results. The only previous account of experiments upon the strength of cotton with which I am acquainted is contained in the first volume of the 'Transactions of the Industrial Society of Mulhouse,' being read before that Society on the 20th December, 1826, by M. Josue Heilmann. His method consisted in taking threads spun from various kinds of cotton, and ascertaining the weight required to break them in some kind of yarn-testing apparatus, and then counting by the microscope the number of hairs contained in the

threads. These experiments, being repeated many times, gave him for

Louisiana cotton	....	$2\frac{1}{2}$	grammes.
Jumel ,,	....	$4\frac{1}{3}$	,,
Georgian ,,	long staple	$3\frac{2}{3}$	,,
Georgian ,,	short staple......	$4\frac{1}{5}$	,,

It will be found that these results are considerably below the truth; for this method, which seems ingenious at first sight, is fundamentally fallacious, because a spun yarn or thread made from short hairs like cotton is not like a bundle of wires, the whole lengths of which may coincide, but, on the contrary, is composed of lengths which theoretically never coincide at all, and practically very few do coincide in their whole lengths: but if a very short grip was taken, and if the cotton hairs were uniform in strength throughout their length, the results might come tolerably near; but cotton has not a uniform strength in all parts, and a number of experiments show that this method can never give more than about one-third of the mean strengths of the hairs in a yarn or thread, and generally very much less.

After many tedious trials, I found out a very simple and efficacious way of securing the ends of the cotton hairs. I take some tough thin paper, like letter-copying paper or good tissue paper, and put a thin layer of gum on one side, and, when dry, cut it into slips about half an inch broad; I bend some of the thinnest iron wire into triangles having sides of about half an inch, and cutting the gummed paper into lengths of about an inch, double a length of it over one side of the triangle, with the gummed side inwards, and just moisten it at the bend for a short distance with a camel's-hair pencil, press together, and so secure it on the triangle, leaving, however, the ends free, and forming an open flap of a quarter of an inch or more in length. The cotton hair about to be tested is brought by the forceps

upon a glass plate, and, one end carried by a moistened camel's-hair pencil between the gummed flaps, it is settled in its place, the pencil communicating sufficient moisture, and the flaps are then carefully pressed together with the finger; the loose end is secured in a similar manner to another triangle. This holds them perfectly tight; and, when dry, the triangles can be suspended upon the hooks, or secured in any other way in the testing-apparatus.

It was soon found that cotton hairs differed very much among themselves in tensile strength, and that a cotton hair was not of equal strength throughout; the resistance was at a minimum when the hair was held by its extremities, and the point of maximum resistance was found to vary in different qualities of cotton, but always to lie nearer the seed-end of the cotton-fibre than to its tapering extremity, generally at about two-thirds or three-fifths of the length of the hair from its weaker end. In order to illustrate this physical proof of the varying strength of a single hair in different parts, I give the results of experiments in which one hair has been broken several times in succession, being freshly connected after every break, so long as it was possible to get hold of the remainder of the hair. In one case of Sea Island cotton (54*d.*) I got five breaks from one hair, as follows, the lengths given being those between the points of suspension:—

1st break	1·7 inch,	broke with	18·1	grs. at	0·5	in. from one end.
2nd	,, 1·0	,,	70·9	,,	0·1	,,
3rd	,, 0·8	,,	97·1	,,	0·1	,,
4th	,, 0·5	,,	126·2	,,	0·2	,,
5th	,, 0·2	,,	133·3			

In Edisto Sea Island cotton I succeeded in two instances in getting respectively seven and eight breaks from the hairs; in every case, except in the first break, the fibres broke quite close to one of the fastenings. The particulars are as follows:—

Successive breakings of Edisto Sea Island cotton.

	a		*b*		*a*		*b*
1st break	1·4 long		?	broke with	48·5 grs.		lost
2nd „	1·0	„	1·3	„	68·1	„	29·7
3rd „	0·75	„	1·1	„	73·9	„	28·7
4th „	0·55	„	0·9	„	78·7	„	36·5
5th „	0·45	„	0·7	„	110·9	„	34·1
6th „	0·25	„	0·35	„	132·4	„	36·9
7th „	0·14	„	0·20	„	144·0	„	48·0
8th „	...	„	0·1	„	...	„	65·3

The fibre *a* shows a regular increase of strength; but *b* is quite exceptional, both as to actual and relative strength of its parts: it is nearly of the same strength throughout, and, examined by the microscope, was found to be one of those flat and twisted riband fibres which, though very rare in good cotton, have been, curiously enough, taken by microscopic delineators as the type of cotton hairs. I have put it down because it shows that, even in the case of hairs very deficient in secondary deposit, the strength increases towards a certain medium point in the hair.

I give here tabulated results of successive breakings of four hairs of Uplands cotton; and I draw attention to the fact that the hairs *c* and *d* show some contradictory results, requiring at the early breaks a greater weight than at some succeeding breaks, showing probably that the fibre has been injured by the strain of the first break. This is not, however, the general result; for in a great number of experiments the hairs show a greater resistance the second than the first time, and so on till the last.

Successive breakings of four hairs of Uplands cotton.

c			*d*		
No. of break.	Length.	Grs. weight.	No. of break.	Length.	Grs. weight.
1	1·10	30·7	1	0·8	74·4
2	0·9	27·8	2	0·55	136·3
3	0·57	51·8	3	0·45	112·8
4	0·33	76·3	4	0·275	127·2
5	0·25	68·6	5	0·15	112·8
6	0·20	109·9			

e			*f*		
No. of break.	Length.	Grs. weight.	No. of break.	Length.	Grs. weight.
1	1·12	83·0	1	0·95	128·1
2	0·66	108·9	2	0·63	139·4
3	0·15	131·0	3	0·45	188·4

A good number of instances of second breaks given in the following tables serve to confirm these results, and clearly establish the fact that the cotton hair is strongest at some point lying between the ends; this point varies for different qualities of cotton, and the length of the fibre which has the maximum strength is of different extent: in some qualities of cotton, such as Sea Island, Queensland, Egyptian, and Maranham, the hairs seem of nearly uniform strength over a considerable length, and in breaking them I get a fair number of cases where the point of rupture lies at a nearly equal distance between the points of suspension. In East Indian cottons, on the contrary, the length of hair possessing the maximum power of resistance to rupture seems to be very short indeed—to be nearly a point in fact; and in nearly every case in the experiments these cottons break *at* the point of suspension, no matter how short may be the distance between these points.

It is evident from the foregoing observations that there would not be the slightest use in ascertaining the breaking-weight of cotton hairs without having regard to the place of rupture. I therefore turned my attention to ascertaining the maximum strength of the cotton hairs; and to do this I had to fix upon what I deemed the strongest part of the hair, and make the distance between the points of suspension very short, as short as one-tenth of an inch for the short staple, and ranging about 0·25 inch for the longer staples. I had no other means of determining the presumed strongest part than by an inspection with the naked eye. I laid the fibre on a black glass plate, and could always see the tapering end, and, coming away from that,

fixed generally upon a part more than halfway towards the root-end, sometimes three-fourths of the way, always leaving the tapering end as far as I could; then, by inclosing all the rest of the hair between the gummed papers, I considered I had the strongest part of it to experiment upon. In selecting the hairs to be experimented upon, I endeavoured to get them fairly representative of the bulk, and did not pick or reject any hairs unless obviously injured.

In the tabulated results which follow, I have arranged the hairs in the order of their strengths, and have given the mean of the strengths. I have been careful to put down all the results I got, without regard to any preconceived hypothesis; and, with the tables before the reader, I shall abstain from making any remarks as to their applicability to any practical purposes, except to say that the experiments were not undertaken with any such aim, and are not presented now with any such pretensions. I only claim for them to be good experiments, made with care and accuracy, and, I believe, the very first made upon cotton hair.

Breaking-weights of 19 hairs of Edisto Sea Island cotton. December 1860. Price 2*s.* 2*d.* per lb.

Distance between points of suspension from 0·1 to 0·4 inch.

No.	grs.	No.	grs.	No.	grs.	No.	grs.
1	... 24·0	11	... 62·6	19	... 88·3	5	... 118·5
4	... 36·9	10	... 68·6	17	... 91·6	3	... 120·0
2	... 41·7	18	... 72·9	13	... 102·4	7	... 130·5
14	... 44·6	6	... 73·9	16	... 111·8	12	... 142·5
8	... 55·2	9	... 74·4	15	... 115·2		

Mean breaking-weight = 83·9 grs.

This fibre, which has the longest and evenest staple, is the lowest in strength, and, with one exception, its maximum and minimum hairs stand also lowest in strength.

Breaking-weights of 22 hairs of Sea Island cotton, "good quality." March 28, 1863. Price 54*d.*

Distance between points of suspension 0·1 to 0·45 inch.

No.	grs.	No.	grs.	No.	grs.	No.	grs.
14	45·6	8	78·2	18	91·2	15	108·4
16	62·4	19	81·6	11	91·2	12	120·9
3	64·3	5	82·5	7	91·2	22	124·8
20	72·0	2	86·4	13	96·0	6	125·7
21	76·8	10	87·8	1	96·0	9	132·0
4	76·8	17	88·8				

Mean breaking-weight = 90·0 grs.

This high-priced and long-stapled cotton is of nearly the same mean strength as the last sample, and very low both in mean and maximum strength; but, on the other hand, a great number of hairs have nearly the same strength, which makes it very regular.

Breaking-weights of 20 hairs of Benguela cotton.

Distance between points of suspension from 0·1 to 0·25 in.

No.	grs.	No.	grs.	No.	grs.	No.	grs.
8	40·3	16	78·0	12	99·3	11	121·9
14	48·0	3	88·8	18	104·6	7	124·3
9	51·8	15	88·8	19	108·0	20	128·1
5	72·2	4	92·6	6	108·4	13	158·4
10	73·9	1	94·5	17	112·3	2	218·8

Mean breaking-weight = 100·6 grs.

This cotton is tolerably even in the strength of the hairs, which are of a low average, with but few strong hairs.

Breaking-weights of 21 fibres of Sea Island cotton. December 1860. Price 16*d.* per lb.

Distance between points of suspension 0·1 to 0·4 inch.

No.	grs.	No.	grs.	No.	grs.	No.	grs.
11	45·6	21	76·8	12	112·8	8	122·4
16	52·8	13	78·7	1	114·7	9	153·6
17	52·8	19	86·8	2	115·0	3	158·4
14	58·5	10	89·5	6	120·0	4	164·4
20	62·4	15	91·9	7	120·9	5	203·0
22	74·4						

Mean breaking-weight = 102·6 grs.

This cotton owes its higher average over the other Sea Island samples to the presence of a few strong hairs; it shows a considerable range, with rather sudden leaps in the differences.

Breaking-weights of 23 hairs of Upland cotton. December 1860. Price $6\frac{7}{8}d.$ per lb.

Distance between points of supension from 0·1 to 0·25 in.

No.	grs.	No.	grs.	No.	grs.	No.	grs.
22 ...	43·2	10 ...	74·4	7 ...	101·7	8 ...	136·8
12 ...	48·0	2 ...	82·5	1 ...	102·2	4 ...	151·2
18 ...	52·8	11 ...	89·2	15 ...	129·9	13 ...	151·6
21 ...	57·6	6 ...	93·1	16 ...	129·6	19 ...	170·4
23 ...	60·9	3 ...	94·0	9 ...	129·6	5 ...	212·6
20 ...	63·3	17 ...	96·0	14 ...	134·8		

Mean breaking-weight = 104·5.

The differences between the fibres are tolerably regular until the strongest hairs, which present considerable intervals.

Breaking-weights of 22 hairs of Surat (fair Dhallerah) cotton. Price $17\frac{3}{4}d.$ March 28, 1863.

Distance between points of suspension 0·1 to 0·35 in.

No.	grs.	No.	grs.	No.	grs.	No.	grs.
4 ...	28·8	9 ...	74·8	7 ...	101·7	10 ...	152·6
3 ...	41·7	2 ...	77·7	16 ...	118·5	5 ...	169·9
8 ...	48·0	12 ...	79·2	22 ...	122·4	18 ...	192·0
6 ...	55·2	1 ...	85·9	17 ...	123·2	20 ...	193·4
13 ...	56·1	19 ...	91·2	11 ...	139·2	21 ...	215·5
14 ...	70·5	15 ...	92·1				

Mean breaking-weight = 105·8 grs.

This is a very difficult cotton to get a reliable average from, on account of its sudden tapering off in both directions from the strongest point: the shorter the distance between the points of suspension, the higher the results.

Breaking-weight of 19 hairs of Maranham cotton.
December 1860. Price 8½*d.* per lb.

Distance between points of suspension, from 0·25 to 0·4 in.

No.	grs.	No.	grs.	No.	grs.	No.	grs.
14	36·9	17	74·4	11	103·6	8	141·1
9	54·2	19	78·7	15	108·4	7	151·2
5	70·0	2	81·6	6	112·3	13	156·9
10	71·0	3	91·6	1	122·4	18	187·2
16	71·5	12	93·6	4	122·4		

Mean breaking-weight = 107·1 grs.

This sample of Maranham seems greatly inferior in strength to another sample recorded later on. Upon referring to the note-book, I find these hairs were suspended with considerable intervals between the supports, while in the other sample the interval was kept as near as possible to 0·1 inch.

Breaking-weights of 21 hairs of "fair Egyptian" cotton.
March 28, 1863. Price 22*d.* per lb.

Distance between points of suspension 0·1 inch.

No.	grs.	No.	grs.	No.	grs.
6	52·8	21	96·0	7	129·6
11	55·6	14	106·0	16	132·0
19	55·6	1	107·5	10	135·3
18	60·4	15	108·0	13	141·6
9	75·3	2	120·4	5	144·0
12	93·6	3	128·6	8	156·9
17	83·0	20	129·1	4	157·9

Mean breaking-weight = 108·0 grs.

Although the maximum strength is low in this sample compared with some of the preceding samples, the general average is brought up by a good number of medium and strong hairs.

Breaking-weights of 22 hairs of Mobile cotton.
December 1860. Price $6\frac{7}{8}d.$ per lb.

Distance between points of suspension from 0·1 to 0·25 inch.

No.	grs.	No.	grs.	No.	grs.	No.	grs.
6	33·6	13	96·0	17	134·4	1	155·0
8	53·3	12	96·0	4	137·2	9	158·8
11	64·3	19	105·6	20	138·0	21	163·2
18	88·8	14	110·4	10	144·0	7	165·1
3	91·2	5	129·1	2	153·1	22	172·3
16	93·6	15	131·5				

Mean breaking-weight = 118·8 grs.

There is a tolerably regular gradation in the differences here, but the range is very great.

Breaking-weights of 14 hairs of Egyptian cotton.
December 1860. Price $9\frac{1}{4}d.$ to $9\frac{1}{2}d.$ per lb.

Distance between points of suspension from 0·2 to 0·4 inch.

No.	grs.	No.	grs.	No.	grs.	No.	grs.
8	77·7	9	98·8	12	136·3	3	169·4
10	77·7	7	117·1	11	147·8	4	185·2
6	81·6	2	126·2	14	154·0	5	191·0
1	87·8	13	130·5				

Mean breaking-weight = 127·2 grs.

Although the distances between the points of suspension in these experiments was at least twice as great as in the previous sample of Egyptian, the results come out higher in every way, proving at once a more regular and stronger fibre.

Breaking-weights for 19 hairs of "good middling Orleans" cotton, at $22\frac{1}{2}d.$ per lb. March 28, 1863.

One-tenth of an inch between points of suspension.

No.	grs.	No.	grs.	No.	grs.	No.	grs.
11	60·5	8	79·2	5	137·5	10	240·9
13	68·1	7	85·1	17	147·8	9	248·6
2	73·4	14	87·8	1	153·1	12	266·4
3	74·9	18	86·4	16	179·0	19	289·4
6	74·9	15	117·6	4	185·3		

Mean breaking-weight = 139·7 grs.

A very great range between the lowest and highest numbers, and very irregular intervals between succeeding fibres. The strongest hair yet tested is in this group.

Breaking-weights of 24 hairs of Pernambuco cotton. March 28, 1863. Price 23*d.* per lb.

Distance between points of suspension 0·3 to 0·7 inch.

No.	grs.	No.	grs.	No.	grs.	No.	grs.
21 ...	72·0	10 ...	108·9	25 ...	138·2	9 ...	158·8
24 ...	72·0	8 ...	122·4	17 ...	139·2	19 ...	174·7
23 ...	79·2	6 ...	126·0	7 ...	143·5	16 ...	193·4
11 ...	90·2	5 ...	129·1	22 ...	144·0	18 ...	228·0
12 ...	96·0	3 ...	131·0	14 ...	144·0	13 ...	243·0
1 ...	100·7	4 ...	132·0	15 ...	147·6	20 ...	251·1

Mean breaking-weight = 140·2 grs.

In this sample, which was one of the earliest I experimented upon in this series, the points of suspension were at considerable distances—in the four last numbers, respectively, 0·4, 0·4, 0·5, and 0·3 inch; notwithstanding, the mean results are high, and the differences pretty regular.

Breaking-weights of 17 hairs of Surat (Dhallerah) cotton. Price, December 1860, 5¼*d.* per lb.

Distance between points of suspension 0·1 to 0·2 inch.

No.	grs.	No.	grs.	No.	grs.	No.	grs.
13 ...	48·0	1 ...	119·5	9 ...	138·2	7 ...	188·6
15 ...	74·4	11 ...	120·4	17 ...	164·1	6 ...	221·7
16 ...	78·0	8 ...	127·2	3 ...	175·2	4 ...	230·8
2 ...	88·3	5 ...	132·0	14 ...	180·9	12 ...	236·6
10 ...	88·5						

Mean breaking-weight = 141·9 grs.

This sample presents a considerable number of strong hairs; it stands considerably higher than the Surat of similar name; the hairs have a bristly stiff character.

Breaking-weights of 18 hairs of Maranham cotton. March 28, 1863. Price *d.* per lb.

Distance between points of suspension 0·1 inch.

No.	grs.	No.	grs.	No.	grs.	No.	grs.
2 ...	88·8	15 ...	126·2	16 ...	141·6	3 ...	170·4
9 ...	96·0	17 ...	127·6	8 ...	142·3	10 ...	173·7
1 ...	96·0	6 ...	128·6	18 ...	157·4	4 ...	228·4
13 ...	105·1	7 ...	133·4	5 ...	168·0	12 ...	242·4
14 ...	112·3	11 ...	134·8				

Mean breaking-weight = 142·9 grs.

The short distance between the points of suspension may perhaps explain why this sample stands higher than the other sample of Maranham; but it may be also an actual difference.

Breaking-weights of 22 hairs of New Orleans cotton. December 1860. Price $7\frac{1}{8}$*d.* per lb.

Distance between points of suspension from 0·1 to 0·2 inch.

No.	grs.	No.	grs.	No.	grs.	No.	grs.
8 ...	81·6	19 ...	106·5	12 ...	139·2	22 ...	202·5
20 ...	86·4	10 ...	110·4	16 ...	153·6	4 ...	212·2
5 ...	91·2	13 ...	112·8	3 ...	187·2	9 ...	214·0
11 ...	96·0	7 ...	112·8	1 ...	192·0	2 ...	232·0
6 ...	96·0	18 ...	122·8	14 ...	197·7	17 ...	264·0
15 ...	103·2	21 ...	136·8				

Mean breaking-weight = 147·7 grs.

This sample of Orleans does not differ greatly from the preceding sample either in mean or maximum results.

Breaking-weights of 21 hairs of Queensland cotton.

Distance between points of suspension from 0·2 to 0·4 inch.

No.	grs.	No.	grs.	No.	grs.	No.	grs.
14 ...	72·0	11 ...	110·4	7 ...	148·3	2 ...	197·7
16 ...	90·2	17 ...	121·4	3 ...	170·4	18 ...	197·7
10 ...	93·6	12 ...	132·4	4 ...	178·0	21 ...	193·0
19 ...	96·0	1 ...	136·8	8 ...	179·0	6 ...	201·2
15 ...	96·0	20 ...	144·0	13 ...	189·6	5 ...	246·2
9 ...	108·0						

Mean breaking-weight = 147·6 grs.

This cotton is an extraordinary instance of a long staple, and at the same time a very strong cotton: it stands out in strong contrast to the other long-staple cottons of similar length of hair.

Breaking-weights of 20 hairs "middling Comptah" Surat cotton. March 28, 1863. Price 15*d.* per lb.

Distance between points of suspension 0·1 to 0·2 inch.

No.	grs.	No.	grs.	No.	grs.	No.	grs.
1 ...	55·2	3 ...	123·8	19 ...	182·4	14 ...	199·2
2 ...	77·7	17 ...	144·0	15 ...	188·1	9 ...	202·0
6 ...	89·2	13 ...	150·2	16 ...	192·0	12 ...	203·5
4 ...	95·1	11 ...	169·4	18 ...	192·0	7 ...	250·0
8 ...	113·7	5 ...	176·1	20 ...	192·0	10 ...	280·2

Mean breaking-weight = 163·7 grs.

This cotton has a great number of short, stiff, strong hairs, which bring up the mean, and place it the highest in the list.

Mean breaking-weight, and breaking-weight of strongest hair in samples.

	Mean. grs.	Maximum. grs.
Edisto Sea Island	83·9	142·5
Sea Island "good quality"	90·0	132·0
Benguela	100·6	218·8
Sea Island Cotton	102·6	203·0
Upland	104·5	212·6
Surat, "fair Dhallerah"	105·8	215·5
Maranham	107·1	187·2
Egyptian, "fair"	108·0	157·9
Mobile	118·8	172·3
Egyptian	127·2	191·0
Orleans, "good middling"	139·7	289·4
Pernambuco	140·2	251·1
Surat "Dhallerah"	141·9	236·6
Maranham, "good middling"	142·9	242·4
Orleans	147·7	264·0
Queensland	147·6	246·2
Surat, "middling Comptah"	163·7	280·2

XXX. *Inquiry into the question, Whether Excess or Deficiency of Temperature during part of the year is usually compensated during the remainder of the same year?* By G. V. Vernon, Esq., F.R.A.S., M.B.M.S.

Read before the Physical and Mathematical Section, January 7th, 1864.

A theory having recently been promulgated, that excess or deficiency of temperature during some of the months of any particular year is usually compensated by a corresponding deficiency or excess during the remainder of the same year, the subjoined investigation has been undertaken in order to see what are the facts during a long period of years.

The observations of mean temperature made use of are those given by Mr. Glaisher for Greenwich Observatory from 1771 to 1849, published in the Philosophical Transactions for 1850; and, in addition, the values from 1850 to 1862 are those given in the Greenwich Observations. This series embraces in all 92 years, and is the only trustworthy series for so long a period to be found in this country. The method of investigation has been as follows:—The mean temperature of each month has been compared with its average for the entire period, and its difference from the mean found; in this manner we obtain twelve values for each year, part positive and part negative. The positive values are added together, also the negative ones, and the two amounts thus obtained are given in columns 2 and 3 of Table I. appended to this paper. We thus get two amounts, which tolerably represent the relative proportions of the mean temperature above or below the average during each year. In columns 4 and 5 the excess or deficiency of the positive or negative values in columns 2 and 3 are given, the amount being placed in the

positive or negative column according as the positive or negative values are in excess for the year. If the theory alluded to were correct, the figures in these two columns ought to be zero. The 6th and 7th columns give the number of months above or below the average in each year respectively.

During the 92 years there were 23 in which the excess or deficiency of temperature found by summing up the monthly differences amounted to over 20° for the year. There were 28 years in which this excess amounted to from 10° to 20°. There were 19 years in which it amounted to at least 5°, and 22 in which the amount was less than 5°.

There are individual years in which the excess of temperature during one part of the year is nearly compensated by a corresponding deficiency during the remainder of the year; but these years are very few in number, there being only 5 in which the amount does not exceed 5°. Taking the entire period, the observations distinctly prove that, during a series of years, excess or deficiency of temperature is not generally compensated during the same year.

Table II. shows the number of years which have a given number of months above or below the average.

This Table is to be read as follows:—For example, taking 4 months, we have 4 years with 4 months out of the 12 above the average; 11 years with 4 months below the average, and 15 years with 4 months above or below the average, taken together.

In order to show the variations better, I have traced the figures given in columns 4 and 5 of Table I., for each year, in the form of a curve (Plate XIII.), taking the years as abscissæ, and the number of degrees excess or deficiency as ordinates.

The central horizontal line in the diagram represents what the curve would become if the theory propounded

were really true, as then all the variations would vanish. The very irregular nature of these variations shows how little dependence can be placed upon results deduced from the observations of a few years, and how useless it is to attempt making empirical laws in the present state of our knowledge.

The period from 1781 to 1791 appears to have been that in which the variations below the average were at their maximum, every year but 1781 being below the average. From 1841 to 1851 there was only 1 year below the average, and from 1851 to 1861 only 2 years below the average.

A careful examination of the entire period, as shown in the diagram, very clearly points out that these variations obey no law of periodicity. That the variations do not balance one another during the same year is very evident.

TABLE I.

Year.	Sum of + differences.	Sum of − differences.	Excess of + readings.	Excess of − readings.	Number of months above the average temperature.	Number of months below the average temperature.
	°	°	°	°		
1771.	4·5	39·6	......	35·1	2	10
1772.	6·2	20·8	......	14·6	4	8
1773.	1·6	22·5	......	20·9	2	9*
1774.	7·5	12·9	......	5·4	5	7
1775.	23·9	3·3	20·6	......	10	2
1776.	12·3	12·5	......	0·2	9	3
1777.	10·7	11·7	......	1·0	5	7
1778.	21·7	10·5	11·2	......	7	5
1779.	35·7	1·4	34·3	......	9	2*
1780.	23·7	17·2	6·5	......	7	5
1781.	18·9	1·5	17·4	......	10	2
1782.	3·2	36·7	......	33·5	1	11
1783.	9·5	13·3	......	3·8	7	5
1784.	5·2	44·0	......	38·8	2	10
1785.	3·7	26·1	......	22·4	5	7
1786.	1·8	32·1	......	30·3	2	10
1787.	5·7	8·2	......	2·5	6	6
1788.	10·3	15·8	......	5·5	5	7
1789.	5·1	24·3	......	19·2	3	9
1790.	8·6	11·6	......	3·0	5	6*
1791.	10·4	13·2	......	2·8	6	6

* One month exactly the average.

TABLE I. (*continued*).

Year.	Sum of + differences.	Sum of − differences.	Excess of + readings.	Excess of − readings.	Number of months above the average temperature.	Number of months below the average temperature.
	°	°	°	°		
1792.	8·1	12·4	......	4·3	5	7
1793.	7·8	17·4	......	9·6	4	8
1794.	18·7	11·5	7·2	......	5	7
1795.	14·9	27·9	......	13·0	4	8
1796.	16·4	22·9	......	6·5	4	8
1797.	4·4	17·9	......	13·5	2	10
1798.	11·5	8·1	3·4	......	7	5
1799	0·5	29·6	......	29·1	1	11
1800.	12·0	12·6	......	0·6	6	6
1801.	14·7	6·0	8·7	......	8	4
1802.	9·0	13·3	......	4·3	6	6
1803.	11·6	13·3	......	1·7	5	7
1804.	21·9	8·2	13·7	......	7	5
1805.	6·5	14·7	......	8·2	5	7
1806.	28·9	3·0	25·9	......	9	3
1807.	14·3	13·6	0·7	......	6	6
1808.	13·7	15·9	......	2·2	5	6*
1809.	13·2	11·8	1·4	......	5	7
1810.	8·9	4·8	4·1	......	7	4*
1811.	21·3	5·8	15·5	......	7	5
1812.	3·6	25·9	......	22·3	2	10
1813.	5·6	19·0	......	13·4	2	10
1814.	4·7	34·6	......	29·9	2	10
1815.	18·2	10·6	7·6	......	6	5*
1816.	5·1	27·8	......	22·7	3	9
1817.	14·2	22·0	......	7·8	5	7
1818.	32·1	2·6	29·5	......	7	3†
1819.	18·9	6·8	12·1	......	8	4
1820.	5·1	16·8	......	11·7	3	9
1821.	24·6	12·9	11·7	......	8	4
1822.	34·9	2·7	32·1	......	10	2
1823.	3·7	15·5	......	11·8	3	9
1824.	11·6	11·9	......	0·3	6	6
1825.	19·7	3·7	16·0	......	9	3
1826.	27·8	8·8	19·0	......	8	3*
1827.	14·0	11·7	2·3	......	7	5
1828.	23·2	1·5	21·7	......	11	1
1829.	3·1	23·8	......	20·7	3	9
1830.	14·9	20·7	......	5·8	6	6
1831.	26·7	1·3	25·4	......	11	1
1832.	11·9	2·9	9·0	......	8	4
1833.	19·7	12·0	7·7	......	5	7
1834.	32·9	0·7	32·2	......	11	1
1835.	15·7	5·2	10·5	......	10	2
1836.	7·9	11·0	......	3·1	6	6
1837.	7·4	19·5	......	12·1	5	6*
1838.	1·3	24·3	......	23·0	2	10

* One month exactly the average. † Two months exactly the average.

TABLE I. (*continued*).

Year.	Sum of + differences.	Sum of − differences.	Excess of + readings.	Excess of − readings.	Number of months above the average temperature.	Number of months below the average temperature.
	°	°	°	°		
1839.	6·2	13·2	......	7·0	5	7
1840.	10·4	16·9	......	6·5	6	6
1841.	14·8	10·6	4·2	......	6	5*
1842.	23·7	8·3	15·4	......	8	4
1843.	18·9	6·0	12·9	......	7	5
1844.	15·5	11·6	3·9	......	9	3
1845.	13·1	21·8	......	8·7	6	6
1846.	41·3	5·9	35·4	......	11	1
1847.	21·7	5·8	15·9	......	7	4*
1848.	24·8	5·6	19·2	......	8	4
1849.	21·9	2·6	19·3	......	10	2
1850.	17·0	6·9	10·1	......	7	5
1851.	19·0	8·6	10·4	......	8	4
1852.	33·3	3·3	30·0	......	10	2
1853.	8·5	16·0	......	7·5	3	9
1854.	16·1	5·0	11·1	......	9	3
1855.	4·9	20·7	......	15·8	5	6*
1856.	16·2	11·1	5·1	......	7	5
1857.	32·8	0·0	32·8	......	11	0*
1858.	19·1	7·9	11·2	......	8	4
1859.	31·7	2·5	29·2	......	10	2
1860.	5·4	23·3	......	17·9	3	9
1861.	19·2	5·9	13·3	......	7	5
1862.	22·6	8·5	14·1	......	8	4

TABLE II.

Number of months.	Years above the average.	Years below the average.	Years both above and below the average.
0	0	1	1
1	2	4	6
2	9	8	17
3	7	7	14
4	4	11	15
5	16	13	29
6	12	14	26
7	14	12	26
8	10	4	17
9	6	8	14
10	7	8	15
11	5	2	7
12	0	0	0

* One month exactly the average.

XXXI. *Examination of the Truth of the Assertion, that when November has a Mean Temperature above the average, it is usually followed by excessive Cold between December and March following.* By G. V. Vernon, F.R.A.S., M.B.M.S.

Read before the Physical and Mathematical Section, January 7th, 1864.

In the Table accompanying this paper I have tabulated all the Novembers from 1771 to 1861 which had a mean temperature above the average, and in the columns adjoining have given the differences between the mean temperatures of the four following months of December, January, February, and March and their mean values.

We find, by classifying the months above and below the average, the following figures:—

Months.	Number of months above the average.	Number of months below the average.
December	25	15
January	22	19
February	21	20
March	23	16
Sum	91	70

or 91 months above the average against 70 months below the average, following a November with a mean temperature above the average.

Out of the entire series, there are 6 years in which a November above the average was succeeded by 4 consecutive months also above the average temperature, and only 2 years in which a warm November was followed by 4 consecutive months below the average.

In place of a warm November preceding excessive cold, we find that in most of the years in which severe frosts

have occurred early in the year, the November previous has had a mean temperature below the average.

November 1784 had a temperature 1°·7 below the average, succeeded by December 7°·8 below, January 0°·4 above, February 7°·8 below, and March 7°·0 below the average. The great frost, which set in fiercely upon the 6th January 1814, was likewise preceded by a November 2°·2 below the average, December 2°·2 below the average; January was 8°·8 below, February 4°·2 below, and March 5°·8 below the average.

The cold period in January and February 1838 was also preceded by a November 1°·3 below the average, and December 2°·4 above the average; January 1838 was 6°·8 below, and February 5°·3 below the average temperature.

Careful investigation of the mean monthly temperatures for the long period made use of shows that no safe conclusions of any kind can be based upon the character of any particular month.

In conclusion, I may state that cold winters succeeding a warm November were very few in number, and in most cases these winters were preceded by a November not much above the average temperature, as in 1783, 1794, and 1799, when the mean temperature of November was only 0°·5, 0°·9, and 0°·5 respectively above the mean.

November 1822 and 1846 were the only two Novembers much above the average which were followed by a cold period immediately afterwards.

Years in which November was above the average.	November Excess.	December following.	January following.	February following.	March following.
	°	°	°	°	°
1772.	+1·0	+0·7	+1·2	−3·3	0·0
1776.	+0·3	+1·3	−1·8	−2·4	+3·7
1777.	+1·3	−3·0	−0·9	−2·6	−0·8
1778.	+2·3	+4·0	−0·9	+7·1	+6·1
1783.	+0·1	−3·8	−6·5	−6·3	−3·2
1792.	+0·8	+1·2	−0·4	+1·5	+1·2
1793.	+0·5	+2·2	−2·4	+6·5	+3·4
1794.	+0·9	−2·0	−11·8	−4·1	−2·3
1799.	+0·5	−6·0	+1·2	−4·1	−3·4
1804.	+1·7	−3·2	−1·2	+0·5	+1·1
1806.	+5·0	+8·0	+1·0	+1·8	−3·9
1808.	+1·5	−2·5	−0·3	+5·9	+1·7
1810.	+0·4	−0·2	−2·9	+1·9	+2·5
1811.	+2·8	−0·2	+0·2	+3·4	−2·5
1817.	+4·5	−1·7	+3·6	−2·4	0·0
1818.	+6·8	0·0	+4·4	+1·8	+3·1
1821.	+5·2	+5·5	+4·1	+5·1	+6·4
1822.	+5·8	−2·4	−3·9	−0·1	−1·1
1823.	+0·6	+1·1	+1·7	−2·0	−1·4
1824.	+3·8	+3·0	+2·7	−0·1	−2·4
1828.	+1·9	+5·7	−4·0	+0·2	−1·9
1830.	+2·0	−3·9	−1·3	+3·0	+3·0
1831.	+1·9	+3·2	+1·6	−1·3	−0·4
1832.	+1·3	+3·6	−1·2	+4·2	−3·3
1833.	+1·1	+5·8	+8·7	+2·0	+3·1
1834.	+1·7	+2·2	+2·3	+3·0	+0·1
1835.	+0·6	−3·9	+1·5	−1·3	+2·8
1839.	+2·3	+0·8	+3·3	−0·1	−3·3
1840.	+1·0	−5·5	−2·1	−2·9	+5·3
1841.	+0·3	+1·7	−2·8	+2·6	+4·0
1842.	+0·4	+6·2	+4·2	−2·2	+2·0
1843.	+1·4	+5·1	+3·4	−3·0	+0·6
1844.	+1·6	−5·8	+2·6	−5·5	−5·7
1845.	+3·4	+2·9	+8·0	+5·7	+2·4
1846.	+3·6	−5·9	−0·6	−2·8	+0·1
1847.	+4·5	+4·0	−1·1	+5·2	+2·9
1848.	+1·4	+5·2	+4·4	+5·0	+1·6
1849.	+1·7	+0·3	−2·0	+6·5	−1·0
1850.	+4·1	+1·8	+7·2	+1·9	+1·7
1852.	+6·5	+8·8	+6·7	−4·9	−2·4
1857.	+3·4	+6·3	+1·8	−3·6	+0·5

XXXII. *On the Height and Order of Succession of Waves, as observed off the Cape of Good Hope.* By Thomas Heelis, Esq., F.R.A.S.

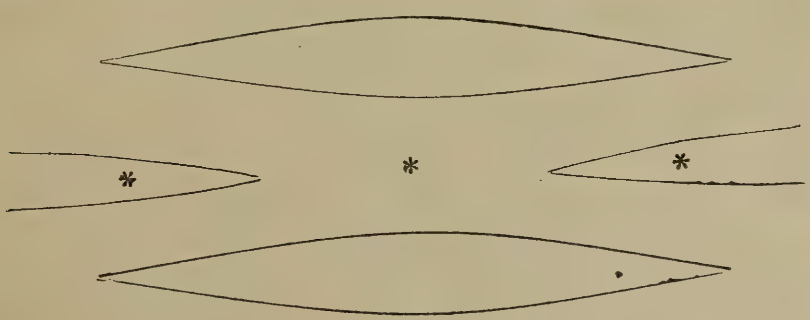
Read before the Physical and Mathematical Section, April 2nd, 1863.

The following observations, almost entirely of the height of waves, were made in the ship 'City of Pekin,' in the year 1862, on a passage from Calcutta to London. The position of the ship is given for the noon of each day of observation. The estimations were made by noting the apparent altitude of each wave above the trough when close to the ship, the eye being eighteen feet above the level of the water. All measurements or estimations of height are from the trough of the sea. The above height shows what observations are most to be depended upon; but I do not think that in any case the height is overstated. No broken wave-tops were estimated, except when it is expressly stated that such was the case; but the heights noted were those of the highest waves observed.

It might be thought that a ship was the best place for this kind of observation; but such, if the ship be on a wind, is far from being the case. Suppose a ship to be brought to the wind under small sail; when hove to, or what is technically called head-reaching, she will, if there be no swell running from a different direction to that from which the wind is blowing, cross the ridges of water at an angle of about 75°, with a speed of two or three knots per hour. In this situation, if a gale be blowing hard, the complications of the waves are so great that it is hopeless to attempt to observe anything beyond the 200 feet or so of the ship's length. This induces an error in the observations, arising from the fact that waves have an origin from which they gradually increase in size (longitudinally) to the crest, and

thence diminish until they are lost. Thus a wave, marked at the ship as small, may be seen at a distance from her to have a crest equal in height to one which would be marked at the ship as large; but if the eye be suffered to range away in quest of the crests of passing waves, the order of their size at the ship will be lost.

Beginning with a wave larger than those ordinarily passing the ship at the time, I have generally found that it is followed at a short distance by another equally large. Between these two often appears a smaller one, which, if it be watched over the bows or stern of the ship, will be seen to be the spur at the origin or end of a larger one, whose crest may be a mile off, the system being shown in the accompanying rough drawing, in which the thicker parts of the figures represent the greater height of the waves, it being understood that in the drawing the breadth (or height) of the wave is exaggerated in proportion to its length, the object being merely to show how an apparently small wave is situated between two larger ones,



so that the line * * * is either a surface of little undulation, bounded by parts of four large waves, or will be crossed by the ship on the lower spur of a wave whose apex is beyond or astern of her.

Thus it appears that measurements of large waves from a ship do not measure so much larger undulations than usual of the whole surface of the water, as points and times at which the summits or crests of waves happen to

coincide within the limits of the ship's motion during their passage.

This being premised, the following comparisons of wave-magnitudes, and estimations of their speeds, although few in number, may not be without interest.

All the observations here mentioned were made during moderate or strong gales of wind.

The comparisons are arranged in three columns or lines, one under the other, the uppermost one containing the highest waves observed, with their altitudes when recorded, the second those of medium height, and the third the small ones. The numbers indicate the order of the waves in the series observed; so that the whole will be in the nature of a rough curve.

13th July 1862.

1st Set.

Latitude 35° 33′ S.; longitude 22° 0′ E.

Large...	1 (25 feet),				11, 12,		14, 15, 16.
Medium			5, 6,				
Small...		2, 3, 4,		7, 8, 9, 10,		13,	

2nd Set.

Large...	1,			7,		12.
Medium			6,			
Small...		2, 3, 4, 5,			8, 9, 10, 11,	

3rd Set.

Large...	1,				12.
Medium			5,		
Small...		2, 3, 4,		6, 7, 8, 9, 10. 11,	

4th Set.

Large...	1, 2 (very large),		6, 7,		24, 25.
Medium		3, 4, 5,			
Small ..				8, 9, 10, 11, 12–23,	

5th Set.

Large...	1, 2,						17–20,	
Medium			6,		9,			
Small...		3, 4, 5,		7, 8,		10, 11, 12, 13, 14, 15, 16,		21–24.

The time occupied in the passage of this last set was measured and found to be four minutes.

Assuming the distance from crest to crest to average 300 feet, this would give a speed of about 20 miles per hour. If the distance be assumed as 350 feet, the speed of the waves measured would be above 23 miles per hour.

In order to check my estimations of the breadth of the troughs between the wave-crests, I took the opportunity, while the ship was being wore round, to estimate again the distance from crest to crest, and to ascertain as well as I could by the time occupied by a crest in passing from the stern to the bows, the speed of the ship at the time being taken into consideration, the velocity with which the crests travel.

I am pretty sure that the distance between the crests lay between 300 and 350 feet. The speed of the ship being about 3 knots, I found that a wave took 8 seconds in passing from one end of the ship to the other, her length being 200 feet. This will give a speed of about 16 knots, or nearly 20 statute miles per hour, agreeing sufficiently well (the difficulty of the observation being taken into account) with the speed computed on the estimation of the distance between the crests being 300 feet.

In the above sets no waves were noted as large which were not estimated as having an altitude of 25 feet or upwards. The small ones were about 16 and 18 feet, and the very large ones 30 to 34 feet of solid water, no broken crests being measured. The force of the wind at the time of observation was about 8 of the Beaufort scale. It had been blowing 10.

A wave begins as a small one, gradually increases in height and bulk, and in its onward progress grows in magnitude until it attains an altitude at which the crest topples over in foam. From this point it decreases rapidly, and soon ceases to exist, its place being taken by the succeeding

wave which has not yet attained its maximum, or by a fresh one in course of formation. If in a gale of wind a large wave be observed approaching the ship and breaking, she will experience it (unless she be so near to the point at which it has broken as to be involved in the broken water) as a comparatively small one; and on looking to windward, it will be found that after the formation of its foaming crest a wave invariably ceases to dominate over its fellows.

When the crest of a wave has toppled over, it always seems to sink much more rapidly than it rose; and I think (but without having been able to verify my conjecture by exact observation) that its altitudes follow the wave-line curve described by Mr. Scott Russell, and that, a line being supposed to be drawn from the point of its formation to that of its extinction, it will be found that its greatest altitude is attained at the point of maximum of such a curve.

It may also be stated with a considerable degree of confidence, although, as before, not yet determined by exact observation, that the length of any one wave forming part of a ridge measured along its base is proportional to the width of the trough; and from what I have observed I should add that its tendency is to be symmetrical on both sides of the point of maximum, although this is often interfered with by a variety of causes, such as the cross sea of a cyclone, which produces pyramidal waves.

The length of a wave seems to depend upon, and bear a definite relation to, the width of the trough between any two successive waves. It is certain, at any rate, that when the waves are low, and the distances between the ridges short, the waves themselves, measured along the lines of their bases, are short also.

On the 18th July 1862, the ship running fast, with the wind and sea right aft, the force of the wind being 7 of

the Beaufort scale, and many of the crests being above 20 feet in height, I availed myself, in latitude 33° 38′ S., longitude 15° 0′ E., of the opportunities thus afforded of making some further observations on waves.

When a ship is running before the wind, the order of succession of the waves cannot be so well observed as when she is hove to; no sets were therefore taken. On the other hand, the speed of the waves and the breadths of the troughs are better observed when running; and to these points I devoted myself.

With waves of 16 feet in height, and breadth of trough 300 to 350 feet, the ship running 10 knots, I estimated the following times occupied by the waves in passing along the ship's length (200 feet):—4″, 5″, 5″, 6″, 6″, 6″, 7″, 7″, and 8″. With troughs of 250 feet, the ship's speed being 10 knots, I obtained for the times of passage 6″, 8″, and 8″. These numbers will give the following speeds of the waves in nautical miles per hour:—37, 29, 29, 25, 25, 25, 22, 22, and 18, and 25, 18, and 18. The highest waves observed this morning were 18 feet; and their length, measured along their bases, varied from 400 to 500 feet.

The wind slightly increased during the morning. I obtained, when its force according to the Beaufort scale was marked 7–8, the times of passage 6″ and 6″, giving the speeds of the waves 25 nautical miles per hour. Their estimated lengths at this time were from 400 to 500 feet, the widths of the troughs varying from 200 to 350 feet, the ship's speed remaining the same.

In the afternoon (the force of the wind marked 7), the widths of the troughs being 250 to 300 feet, and certainly not exceeding the latter estimation, I obtained for times of passage 5″ and 6″, giving speeds of 29 and 25 nautical miles per hour.

Although some of the discrepancies exhibited by the foregoing estimations evidently arise from errors of estima-

tion, yet I am convinced that the speed of waves in moderate weather does vary. When a wave has overtaken that which precedes it, it will unite with it, and assist in producing the difference of height so often observed.

In some of the cases observed on the 18th July, the speed but slightly exceeds that of the waves measured on the 13th of the same month, although the force of the wind on the 13th was nearly double that of the wind on the 18th, the difference between the forces of the wind denoted by the successive numbers near the end of the Beaufort scale being much greater than those between the lower numbers.

The question suggested by the Admiralty 'Manual of Scientific Inquiry,' whether the height and distance of the ridges vary with the velocity, can best be solved by comparison of distinct series of observations, taken at different times, in or near the same locality, as it is hardly capable of distinct observation contemporaneously with other points of inquiry.

The above remarks are independent of the question whether, in addition to the series of undulations measured, there be not (as circumstances seem to prove that there are) series of undulations which take a longer time than those measured, but which coincide with them at certain longer intervals. During a gale of wind, two or three very large waves will often come together at intervals of ten minutes or a quarter of an hour, or sometimes at even longer intervals, causing the ship to lurch fearfully; but no observations yet made have ever reduced these to any known system or series of undulations.

My observations show that, beyond a certain point, the force of the wind has very little influence in increasing the speed of waves. I do not think that they often run much beyond 25 miles per hour.

XXXIII.—*Observations of the Zodiacal Light.*

By Thomas Heelis, Esq., F.R.A.S.

Read before the Physical and Mathematical Section, March 3rd, 1864.

In the course of two or three voyages which, in the years 1861 and 1862, I was called upon to make, I had opportunities of studying this remarkable phenomenon in latitudes in which it is seen to great advantage, and under circumstances which allowed of a continuous series of observations, such as is seldom, if ever, possible in Europe. I am well aware of the difficulty which such an object presents, and of the different results which will be attained by any two observers in the study of it, and hence I feel considerable diffidence in bringing my observations before the Society; but as I have in all cases taken care to note the lesser rather than the greater limit of the phenomenon, and have compared the observations with a map of the stars which have been used for determining the boundaries of the light, I am not without hopes that the errors of my eye in failing to detect the extreme boundaries may be constant and capable of elimination, and that the excellence of my opportunities will enable me in some small measure to add to what is already known on the subject.

I originally left England in the month of August 1861, on a voyage to Constantinople and Smyrna, and reached England again at the end of September. This voyage only yielded one observation of the light, which was made in Smyrna Bay, and was communicated to the Society soon after my return. I subsequently left England for Calcutta in the middle of November in the same year, making the passage round the Cape of Good Hope. On this passage I obtained no observations of the light; but afterwards, during a voyage from Calcutta to Hong Kong

and back, I obtained a fairly continuous series of observations, to which, on my passage home, also round the Cape of Good Hope, I made various additions. During my voyage home, the late Captain Jacob was on his voyage out to Bombay, *en route* for Poonah; and I have examined his observations, as communicated by Prof. C. P. Smyth to the 'Monthly Notices of the Royal Astronomical Society' in December 1861, and compared such as were made on the same days with my own.

The following Table gives the observed positions of the apex of the light, and its length when one (or the inner) cone only was observed. I have added also to this Table the times and places of observation, and the observations of the inner cone of light in cases in which the envelope has been observed—of which phenomenon more hereafter. In all cases I have used the approximate mean time at ship, taking her position as that determined at the nearest noon. Thus, in cases in which observations were made in early morning and on the following evening, the place of the ship will appear to be the same in both cases; but as this seldom happened except when the weather was very settled, the preceding and subsequent places of the ship will allow of her exact position at the time of observation being estimated with considerable precision. Any attempt to give the places with more precision would have necessitated extracts from the ship's log and the working of the dead reckoning, in every case, to the hour of observation; but this labour seemed needless.

TABLE I.

Date.	Time.	Position of apex.		Length.	Lat. of apex.	Place of observation.
		R. A.	Decl.			
1861.	h m	h m	° ′	° ′	° ′	
Sept. 13.	4 A.M.	7 34	17 0 N.	58 42	−4 40	Smyrna.
1862.						
Feb. 22.	8 P.M.	2 52	12 30	72 1	−4 0	Kedgeree.
23.	8	2 12	16 0	61 19	−2 30	Head of Bay of Bengal.
24.	8	2 52	12 30	70 0	−4 0	17° 52′ N. 90° 50′ E.
25.	7 45	2 52	12 30	69 0	−4 0	14 32 93 22
26.	7 30	2 12	17 30	58 18	+4 0	11 4 96 6
27.	7 30	2 52	12 30	68 0	−4 0	7 37 98 12
Mar. 20.	7 30	3 36	21 0	56 38	+1 40	Off Hong Kong.
22.	8	4 26	23 0	67 8	+1 14	15° 11′ N. 110° 9′ E.
23.	8	4 0	21 0	59 26	+0 35	10 43 109 0
24.	7 30	4 10	15 0	61 19	−6 8	6 25 106 53
25.	8	4 10	15 0	60 21	−6 8	2 53 104 54
28.	7 30	4 16	22 0	58 20	+0 42	Straits of Malacca, west [end.
29.	7 30	4 16	22 0	57 21	+0 42	Off Penang.
30.	7 30	4 16	18 0	56 21	−3 18	8° 0′ N. 97° 41′ E.
31.	7 30	4 16	18 0	55 22	−3 18	11 33 95 3
April 1.	8	4 16	18 0	54 23	−3 18	15 1 93 0
May 23.	8	8 32	20 0	64 0	+1 14	Diamond Harbour.
25.	8	8 32	20 0	62 4	+1 14	20° 13′ N. 88° 35′ E.
June 16.	7	9 32	14 0	55 24	−0 41	3 17 S. 82 35
17.	7	9 32	15 0	54 27	+0 19	4 44 80 52
18.	7	9 32	14 0	53 30	−0 41	7 1 79 20
19.	7	9 32	14 0	52 33	−0 41	9 11 77 25
20.	7	9 32	14 0	51 25	−0 41	11 40 74 35
22.	7	9 44	11 0	53 32	−2 26	14 48 70 5
29.	7 10	9 52	13 0	48 61	+0 13	24 47 53 56
July 16.	6 40	11 12	5 0	53 52	+0 4	35 4 20 30
20.	7	10 52	7 0	46 13	−0 11	28 48 9 54
21.	7	11 12	5 0	49 6	+0 4	27 14 8 26
24.	7	11 28	3 30	50 8	+0 5	22 21 3 17 E.
26.	7 15	11 20	6 0	46 16	+1 49	19 56 0 0
27.	7 P.M.	11 20	4 0	45 8	−0 11	19 44 0 18 W.
28.	4 A.M.	4 40	22 0	54 35	−0 11	19 17 0 40
	7 P.M.	11 20	4 0	44 21	−0 11	
Aug. 2.	4 45 A.M.	4 52	22 0	59 27	−0 19	11 48 10 14
4.	before dawn.	4 32	22 30	62 4	+0 25	8 48 13 45
5.	4 40 A.M.	4 40	22 0	62 6	−0 11	7 17 14 48*
6.	4 40 A.M.	4 40	22 0 N.	63 4	−0 11	5 21 S. 16 4
13.	7 30 P.M.	14 10	12 0 S.	79 25	−2 46	6 9 N. 26 17†
15.	7 30	13 24	11 0	60 32	+2 7	6 19
18.	7 30	14 44	11 0	78 38	−5 0	9 32 26 55
19.	7 30	14 44	11 0	77 40	−5 0	9 48 27 10
20.	7 30	14 44	11 0	76 42	−5 0	10 6 28 15
21.	7 45	14 40	15 0	74 43	−0 42	10 46 29 28
22.	7 30	14 48	13 0	75 46	−3 18	11 13 29 45
23.	8	15 7	13 0	79 50	−4 43	11 7 30 10†
24.	7 30 P.M.	14 44	13 0 S.	72 51	−3 0	11 30 31 0
25.	4 A.M.	4 52	22 0 N.	70 53	−5 25	11 46 32 0†
27.	7 40 P.M.	14 40	15 0 S.	68 56	−0 42	14 43 33 20
	8 15	15 0	14 30	63 58	−2 40	
28.	8 15 P.M.	15 24	14 0	69 3	−4 45	17 7 36 50 W.

* Off Ascension. † D. R.

The variations in the latitude of the apex shown in the above Table are remarkable. Some are no doubt due to errors of observation, although every care was taken to guard against such. If we refer to the observations of Cassini, given by Mr. Jones at the end of those made by him during the United States Expedition to Japan, we shall find that, although the body of the light is seldom equally distributed on each side of the ecliptic, the apex has only on two occasions decided latitude, and that on those two occasions the latitudes are N. The observations are 21st April 1685, in which the apex has a latitude of 5° N., and the 15th October 1687, in which the latitude is about 4° N. These observations seem to have been made at Paris, and Cassini mentions that the apex had N. latitude towards the end of April 1683, and that that circumstance had caused him to think that its plane nearly coincided with the sun's equator. Mr. Jones compares the first of these observations with a set made by him on the 21st April 1854 in lat. 34° 40′ N., long. 138° 59′ E., from the chart of which it appears that at $7^h\ 52^m$ the apex was nearly in the position which it occupies in Cassini's observations, but that it had greater N. latitude (about 6°), and that by 9 P.M. this apex was visible some 15° further, but that its N. latitude had decreased so as to be little more than 2°. He also compares Cassini's observation of the 15th October 1687 with two of his own, made on the 16th and 20th October 1854; but on reference to the plates it will be found that Mr. Jones's observation of the 16th is incomplete, the apex being merged in the Milky Way. Enough, however, is shown to assure us that the great mass of the light is on the south side of the ecliptic, whereas in the observation of Cassini it is on the north. The American observation was made in 33° 16′ N. long., 177° 28′ W. The American observation of the 20th October 1854 is also defective, on account of the apex being merged

in the Milky Way. The mass of the light, however, appears to be to the north of the ecliptic. The observation was made in 28° 5′ N., 164° 24′ W. Mr. Jones states, as part of his results, that when he was north of the ecliptic the main body of the light was on the north side of that line, and *vice versâ*. Now, of eleven observations by Cassini, which he gives, all apparently made at Paris, seven show the main body of the light on the north side of the ecliptic and four do not; and there are many exceptions to the supposed rule in Mr. Jones's own work. Three of the observations by Cassini have already been compared with Mr. Jones's results. The others show the following results. The positions of Cassini and Mr. Jones were, during all the observations compared, north of the ecliptic. The signs + or − mean that the body of the light was observed to lie north or south of the ecliptic.

	Cassini.	Jones.		Cassini.	Jones.
February	−	+	November	+	+
March	−	+	December	+	+
September	−	−	December	+	+
September	+	−	March	−	−
			November	+	+

Surely these results look more like a change of position depending on the time of year than on the place of observation. My own observations, so far as the position of the axis implies the position of the main body of light, go to this view of the case, although not as distinctly as might be wished. The gradual diminution of the latitude until July is remarkable; but I am at present unable to account for the subsequent increase being of the same sign, especially as the observations made in the Mediterranean in 1863, tabulated hereafter, have an opposite sign.

Table I. contains the observed lengths and positions of the apex of what I shall hereafter call, for the sake of distinction, the inner cone. On the 16th June 1862 traces of a much fainter envelope surrounding the inner cone, and extending

at the apex much beyond it, were first observed. I find the observation recorded in my note-book in the following terms, written down at the time, and which may be taken to be a fair description of the peculiarities noted in the observations of the faint envelope:—"There seems a fainter kind of luminous envelope surrounding the true light, as if it were cigar-shaped, in layers. This outer envelope is less bright than the inner [cone], especially near the apex, and near the horizon it tones off into the other, thus accounting for the large breadth assigned to the light near the horizon. At the base the two envelopes are undistinguishably mixed. I have often noticed this before, but have not included the envelope in my measures in cases in which I could distinguish it from the true light. The luminous envelope this evening was, at Præsepe, about two-thirds of the brightness of the true light." The light of this envelope was by no means equable. It seldom appeared at all until long after the departure of the twilight permitted observations of the main body of the light, and it generally made its appearance as a faint streak, most frequently on the southern side of the light, and extending from the limb near the apex, apparently overlapping the limb, and extending far beyond the apex. As the night wore on, and the main body of the light sank beneath the horizon, the other limb of the envelope appeared, completing the cone. The main body of the light was hardly ever, near the apex, shaded gradually into the envelope; but the space between the brightest part of the latter and the main body of light was comparatively dark, and gave me the impression of looking into space through a very thin crape veil. I was never able to separate the envelope distinctly from the main body of the light in the few observations which I made in the morning.

The following are the observed lengths of the envelope. The Table is arranged in the same way as that in which

the observed lengths of the main body of the light have already been recorded, omitting the place of the ship, which has been already given :—

TABLE II.

Date.	Time.	R. A.	Decl.	Length.	Lat. of apex.
1862.	h m	h m	° ′	° ′	° ′
June 18.	7 P.M.	10 0	17 0 N.	65 5	+6 30
July 21.	7	12 32	1 0 S.	70 38	+2 34
26.	8	12 32	3 30	65 51	+0 4
27.	7 30	13 0	5 0	72 47	+1 39
28.	8	13 0	5 0	71 50	+1 39
Aug. 13.	8	14 48	11 0	84 26	+5 18
17.	8 15 P.M.	15 16	17 0 S.	78 0	+1 15

This small Table, as well as the preceding one, contains remarkable peculiarities besides those already noticed, for which I am unable to account, and which are also shown in the American observations. I allude to the change of latitude of the apex at different times on the same evening, and also to the fact that the axis of the envelope frequently seems not to be coincident with that of the main body of the light. This change of latitude is shown in the observations of the evening of the 27th of August 1862 in the first Table, and the difference between the latitude of the apex of the envelope and that of the apex of the true light; and that both do not lie in the same plane appears from Table II. In the American observations, both these peculiarities are common. Of the first class may be mentioned at hazard the observations represented in plates 3, 7, 62, 66, 67, 106, 110, 140, 141, 188, notably 201, and still more so 222, where the sign of the latitude is changed in the course of the observations; and of the second class, the observations in the plates numbered 10, 13, 15, 24, 27, 30, 39, and 40. The list of both could be swelled so as to include a large proportion of the plates in the book. This, and deviations of the light from a true figure, which are common, as well as the fact above noticed in the descrip-

tion of the envelope, that one limb of it generally appeared before the other, seem to militate strongly against the idea that the light is caused by a nebulous atmosphere surrounding the sun, but might be explained by the theory of the light being caused by a ring of asteroids.

The light of Jupiter and Saturn, which during most of the observations were very near together, interfered at times considerably with the observations; and I was obliged to take precautions such as hiding the planets, especially Jupiter, behind some intervening object, or to avail myself of a passing cloud, in order to arrive at satisfactory results. On the 17th July especially my note-book records that "The southern limb of the envelope is brighter than the northern; but this is caused by the light of Jupiter; and if the planet be concealed behind any object, the envelope becomes distinctly visible; but it is so delicate an object, that I cannot compare the brightness with anything. As a coarse estimation, I should say that it was not more than one-third of the brightness of the true light. This envelope was visible after the setting of Jupiter, and at times I thought that it extended to the meridian of Spica Virginis." And again, on the 24th July, I find an entry respecting the inner cone as follows:—"Apex apparently not symmetrical, no light appearing near Saturn, so that the top from and towards v Leonis appeared concave; but this may have been caused by the light of Jupiter, then near Saturn." At times no trace of the envelope could be observed to extend beyond the apex of the cone; and its existence on such occasions was only manifested by very delicate shading along the limbs of the latter. At other times, as on the evening of the 27th July, this occurred and was noticed while I was observing the main body of the light, and the extension of the envelope beyond the apex manifested itself as the night wore on.

It seems to me that the phenomena observed are best explained by the hypothesis that the sun forms, with the zodiacal light, a system similar to that of Saturn and his rings. The main body of the light would, according to this hypothesis, be analogous to the broadest of the bright rings of Saturn—the distance between the sun and the inner edge of the ring being so small that the inner edge of the ring sets before the darkness has become sufficiently great to allow of observation, and the main body of the light, or main ring, being separated (as in the case of the rings of Saturn) from the outer envelope or exterior ring, thus accounting for the dark space seen beyond the apex of the main body of the light and between it and the luminous envelope.

It may not be out of place here to say a few words on the distribution of the tracks of meteors in the tropics and southern heavens. It is not unusual to see them fall along the axis of the zodiacal light. I have seen this occur frequently. At Smyrna many small meteors were observed to cross the body of the light in various directions during the observation. On the 22nd March 1862, in latitude 15° 11′ N., long. 110° 9′ E., a meteor of the fourth magnitude, and slightly reddish in colour, was observed to fall from γ Tauri along the southern edge of the light. Again, on the 24th July in the same year, in lat. 22° 21′ S., 2° 17′ E., I find in my note-book that a meteor of the third magnitude, white and tailless, passed just below Saturn, towards the horizon, along the axis of the light. Others might be mentioned. All my observations go to show that the tracks of meteors tend to parallelism with, or to be at right angles to the Milky Way, or to be parallel to the ecliptic; but of course this refers only to meteors which have a cosmical origin, and not to those which are properly atmospheric; and as this topic is foreign to my present subject, I pass from it.

The nature of the light seems to be peculiar. I always noticed that when the eye had been affected by artificial light, as on coming first on deck from the cabin, the light of the Milky Way became clear to the eye long before that emitted by the zodiacal light; but after the eye had been for a short time in darkness, the volume of light frequently produced an effect which I find in my note-book described as half as bright again as the sword-handle of Perseus, as bright as the Milky Way in Argo, &c. I have seen it so bright as to cast quite a beam of light upon the water; and on the 1st April 1862, in lat. 15° 1′ N., long. 93° E., I was able to observe it before the setting of the moon, then 2·2 days old at noon at Greenwich. In spite of all this, I never on any occasion failed to see Præsepe through any part of it at times when it overlaid that object, which was always, when available, used as a test of the intensity of the light.

The following Table contains the results of a few observations made in the Mediterranean in the year 1863. It should be observed that the division of the light into two distinct envelopes has never been observed by me there, and that the lengths and positions given are those of the whole of the light visible. The Table is arranged as before, the position of the ship being given at the nearest or following noon.

Table III.

Date.	Time.	R. A.	Decl.	Length	Lat.	Place of Observation.
1863.	h m	h m	° ′	° ′	° ′	
Sept. 18.	3 30 A.M.	7 35	25 0 N.	63 0	3 25*	Approaching Corfu.
Sept. 20.	3 30	7 12	22 30	70 17	0 6†	41° 52′ N., 16° 52′ E.
Sept. 21.	4	8 24	21 0	54 5	1 43‡	Approaching Ancona.
Oct. 17.	4 A.M.	8 4	25 0	84 43	4 40	Off Cape de Gatta.

* Faint, and at apex ill defined; so that the position assigned to apex may be in error in declination.

† Apex estimated as coincident with δ Geminorum.

‡ Light very faint and dull, definition bad.

The observations of Captain Jacob, as communicated to the Astronomical Society by Professor Smyth, and published in the 'Monthly Notices' for December 1862, only afford two instances of observation on the same days as mine, and therefore directly comparable. Of course the value of this comparison will consist more in the observations of the angle of the light with the ecliptic than in that of the length.

TABLE IV.

Date.	Time.	R. A.	Decl.	Long. of sun	Length	Lat.	Place of Observation.	
1862.	h m	h m	° ′	° ′	° ′	° ′	° ′	° ′
June 16.	7 0	10 5	12 30 N.	85 1	64 4	0 43	26 30 S.	34 0 W.
June 16.	7 0	9 32	14 0	85 1	55 24	−0 41	3 17	82 35 E.
July 16.	6 40	12 57	5 34 S.	113 37	82 19	0 43	29 52	55 19
July 16.	6 40	11 12	5 0	113 37	53 52	0 4	35 4 S.	20 30 E.

Column 5 contains the longitude of the sun. The first and third lines contain the observations made by Captain Jacob, the second and fourth those made by myself. All the other observations communicated by Professor Smyth fall within the period over which my observations extend; but none are made on the same days, except those above cited. His lengths are much greater than mine; but neither of mine on the particular days included the outer envelope.

It appears evident from all the observations, and also from those of Captain Jacob, that great changes occur in the length of light visible, whether these be due to obstructions to vision offered by our own atmosphere or to actual differences. The same peculiarity is notable in the observations made during the American Naval Expedition to Japan. The observer, the chaplain of the ship, often delineates on his charts lengths which I have never seen approached, such as observations of the morning light visible before midnight; but others of his charts show lengths of 55° only.

The great difficulty in such cases as this is, that the observations, even of the same man, at different epochs in a long series of observations are not comparable *inter se*, especially if the observer have in the course of his observations adopted a theory which insensibly biases him. I incline also to think that the light changes its form more than can always be fairly accounted for by differences in our atmosphere; but although I have devoted much time and thought to the subject, both in observing and consulting the observations of others, I am completely at a loss to account for the phenomena observed, upon any theory hitherto broached; nor do I believe that a sufficient number of reliable facts have been collected to allow of any one undertaking the task of forming a theory with any hope of success. The question has been mooted whether the variations observed have any connexion with the solar-spot period; but at present I do not think that this question can be solved, for want of observations. The daring way in which the American observations assign the boundaries of the light, even when it is involved in the Milky Way, and the evident bias which leads their author to observe phenomena which, if true, would bear out his peculiar view, but which have never been observed by any one else although carefully looked for, renders this mass of observations (for there are upwards of 350 charts) very doubtful. I am aware of the numerous chances of error incident to this class of observations, and that my own are by no means free from errors; but I can conscientiously say that they are free from bias.

XXXIV. *Additional Observations on the Drift-deposits and more recent Gravels in the Neighbourhood of Manchester.* By EDWARD HULL, B.A., F.G.S., of the Geological Survey of Great Britain.

Read December 29th, 1863.

DURING the past summer I have been occupied for the most part in an attempt to trace the subdivisions of the Post-pliocene or Drift deposits within the tract of country bounded by the Penine Hills on the east, and by the uplands of Rochdale, Bury, and Bolton-le-Moors on the north, and extending into the Cheshire plain; and I proceed to lay a brief account of the results of this examination before this Society, while the more enlarged details are in course of publication in the Memoirs of the Geological Survey*.

Geologists have for several years been familiar with the classification of these deposits, as laid down by our President, Mr. E. W. Binney, F.R.S.†, and which may be succinctly stated in the order of superposition as follows :—

Recent............	1. Valley-gravel and river-terraces.
Postpliocene or Drift.	2. Forest-sand and gravel of Cheetham Hill, Kersal Moor, &c.
	3. Till, or Boulder-clay.
	4. Sand or gravel, more inconstant, and of less importance than No. 2, and only known in sinkings of wells, &c.

The author of the above classification expressly confined his observations to the neighbourhood of Manchester, to which they are strictly applicable; and in my Memoir

* Geology of the Country around Oldham and the Suburbs of Manchester. 1864.

† "On the Drift-deposits of Manchester and its Neighbourhood," Mem. Lit. and Phil. Soc. vol. viii. 2nd series.

"On the Geology of the Country around Bolton-le-Moors" I accepted without hesitation that classification.

It was my intention, with the assent of the Local Director of the Survey, Professor Ramsay, to ascertain whether these several subdivisions of the Drift and more recent gravels could be followed out over a large tract of country, or were only true as regards the district embraced by Mr. Binney's paper; and I may state at the outset, that the regularity with which the various members of this formation have been found to spread over a tract which may be defined as the rain-basin of the Mersey has far exceeded my expectations, and that the classification of these members points to three different epochs of formation.

The district over which I have surveyed, and mapped each of the three members of the Drift here referred to, extends from Bolton-le-Moors on the north, to Oldham on the east, and Alderley on the south. My colleague, Mr. Green, has continued and verified these divisions still further south, as far at least as Congleton. Thus it may be said that they have been proved to maintain their regularity and order over an area of 600 square miles.

The result of our investigations has obliged us in some measure to modify the arrangement proposed by Mr. Binney. I had not long commenced to trace the upper and lower boundaries of the "Forest Sand" (No. 2), before I discovered that it was overlain by a second formation of Till, or Boulder-clay, quite as important, both in thickness and extent, as that which lies below it (No. 3 above). Now, over the greatest part of the hills of sand to the north of Manchester, this Upper Boulder-clay has been denuded away; but it sets in further to the north-east, in the direction of Oldham, and to the north-west, in the direction of Bolton. Mr. Binney, although he has not included it in his tabular view of the Drift-deposits, was, I believe, aware

of its existence, and mentions it as occurring in the neighbourhood of Cheetham*.

Another modification which we found it necessary to make, had reference to the lower sand (No. 4) underlying the Till in Mr. Binney's classification. We have nowhere been able to discover such a bed *in situ* during our examination; and it is remarkable that in the section of the Drift which was furnished to Mr. Binney as having been proved at St. George's Colliery, Manchester, and where it is stated that this sand and gravel (No. 4) is 10 feet 6 inches in thickness, there is no appearance whatever of it in the neighbouring quarries of Collyhurst, where the Till may be seen reposing directly on the Permian sandstone. I do not, however, wish to deny that there are occasional patches of sand or gravel underlying the Lower Till, because such bands occur in the Till itself. My only object is to remove this member from the dignity of a distinct subdivision of the Drift-series, at least until there is some better evidence of its existence than the reports of well-sinkers, the elasticity of whose system of nomenclature is, unhappily, proverbial. I therefore beg to submit the following classification, which, except in the above-named points, does not differ from that laid down by Mr. Binney:—

Drift and Recent Deposit of the Basin of the Mersey and its Tributaries.

Recent. 1. *Valley-gravel and River-terraces.*

2. *Upper Boulder-clay, or Till.* Bolton Moor, Halshaw Moor, Clifton Moss, Moston, Oldham, Newton Heath, Denton, Cheadle, Hulme, &c.

3. *Middle Sand and Gravel.* Bolton, Pendlebury, Prestwich, Kersal Moor, Heywood, Middleton, Blackley, Gorton,

* I think the explanation of a section given by Mr. Binney, of the sand wedging apparently *into* the Till, will be found in supposing the Upper and Lower Till to meet each other, owing to the thinning away of the sand near the margin.

Stockport, Poynton, Wilmslow, Prestbury, Macclesfield, Crewe, &c.

4. *Lower Boulder-clay, or Till.* Monton, Salford, Manchester, Heaton Norris, &c.

It would be mere repetition were I to attempt to describe these subdivisions of the Drift; and I shall therefore not dwell at any length on the stratigraphical character of these beds, further than to make one or two observations.

The Upper and Lower Boulder-clays are in all respects similar. Of the stones and boulders which they contain, at least two-thirds exhibit marks of glaciation; and there can be no question that they are both subglacial deposits.

Both subdivisions are also laminated or rudely stratified. On this point Professor Ramsay and myself became convinced after a careful examination of many sections, some near Manchester, others along the estuary of the Mersey.

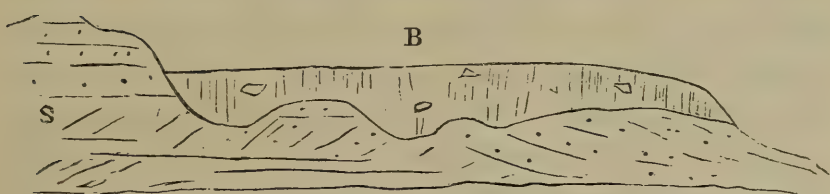
On the other hand, the Middle Sand and Gravel (No. 3) is altogether distinct in this latter respect from the Boulder-clays both above and below it. The pebbles it contains are always water-worn and rounded; and I am persuaded that during its deposition very different physical conditions must have pervaded this part of England from those which obtained the ascendancy during the periods of the Upper and Lower Till. I now pass on to notice certain facts regarding the arrangement of the several members in this district.

Denudation of the Middle Sand.—In confirmation of the views just stated, I may here draw special attention to the evidence afforded of a very extensive denudation of the sand previously to the deposition of the Upper Boulder-clay. The thickness of the sand undergoes the most rapid changes. In some places, as at Kersal Moor for instance, it attains a thickness probably not under 200 feet; and within a distance of not more than 4 miles (that is, at Newton Heath and Openshaw) the thickness is just one-

tenth of this amount, or 20 feet. Indeed, within a less distance than this, the sand dwindles down almost to nothing near St. Luke's, Cheetham Hill. Similar phenomena are observable in many places over the tract we have examined; and I have reason to doubt whether in some places, such as Atherton and Hindley, there is any sand separating the two Boulder-clays from each other.

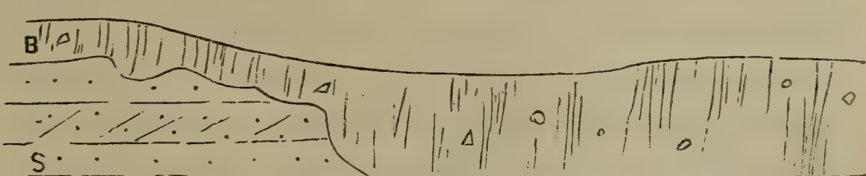
This may be due in some measure to irregularity in the original deposition of these beds; but there is reason to think that it is due in a still greater degree to a subsequent denudation, or removal, of strata which were once deposited with more or less regularity. In confirmation of this view, several instances which came under my notice may be adduced, in which the Upper Till was observed to lie upon an eroded surface of the sand. Out of several I select two in the neighbourhood of Oldham; but similar examples were observed in a pit at Moston Hall, and in a new road-cutting at Whitefield. In some other places,

Fig. 1. *Section at Heyside near Oldham.*
Length of section, 45 yards.



B. Upper Boulder-clay, resting in a hollow denuded in the sand.
S. The Middle Sand underlying the Till, but rising above it at the surface.

Fig. 2. *Section near Chadderton Workhouse.*



B. Upper Boulder-clay, on an eroded surface of the sand S.
The length of this section is about 50 yards, and the depth 6 yards.

however, the superposition of the two formations takes place along a very level and clearly defined line, as may be observed in a large pit at Openshaw and Clayton Hall.

The Lower Boulder-clay, or Till.—Over the district south of the Mersey, the Lower Boulder-clay rarely makes its appearance, the country being overspread by the Upper Till, resting on the sand. The Lower Till, however, may often be traced at the bottom of some of the deeper valleys, such as those of the rivers Dean and Bollin and that of the Tame above Stockport. North of the Mersey, at Stockport, it occupies the tract from Heaton Norris to the Irwell, west of Manchester; and on the opposite side of the river, from Salford to Leigh. It also crops out at the base of the high banks of sand along the river Roch, from Radcliff Bridge upwards for several miles. In the hill-country it seldom or never makes its appearance, as all the Boulder-clay there to be found belongs probably to the upper member of the series.

The Middle Sand.—This division occurs in great strength at Macclesfield, Prestwich, and Poynton. It forms the banks along the valleys of the Bollin and Dean and Bramhall Brook. It has a thickness of 50 feet at Stockport and Heaton Mersey, and from the banks of the Tame all the way to Staleybridge. Traced from Heaton Norris, it forms a band of slightly rising ground by Reddish, Sandfold, Openshaw, Clayton Hall, and Harpurhey to Blackley and Crumpsall, where it swells out considerably. It covers the country for the most part around Middleton, Royton, Heywood, and Rochdale. It forms the high banks along the Irwell and its tributaries, from Pendleton to Bury, Bolton, and up into the hills beyond Sharples, where the gravel becomes of a very local character, the pebbles being principally formed of Millstone-grit; and it forms a bank of rising ground on its southern outcrop, extending from Swinton westward by Worsley towards Ince in Wigan.

It also forms outliers over the Cheshire plain, as at Bowdon and High Leigh. Near its margin it often appears to thin away rapidly, the Upper Till descending to meet the Lower, as in the case at Cheetham Hill, mentioned by Mr. Binney. Such accidents I am disposed to refer to the period of the last denudation of the country, when these post-pliocene deposits were very largely removed by the waters of the retreating sea. The sand being extremely soft and porous, the sea along the margin would penetrate inwards to some distance, and, forming a running sand, might wash it away much more rapidly than the Upper Boulder-clay, which, from its stiff and plastic nature, would to some extent withstand the action of the waves.

The Middle Sand is, unfortunately for its consistency of character, not always free from bands of loam or clay. One of these, which is largely used for brick-making near Prestwich, Heywood, and Rochdale, occurs about the centre of the mass, and divides the sand into two members, the upper of which frequently occurs in detached hillocks. This bed is, however, of very local occurrence, and thins out southward.

It is very probable, if not positively certain, that the Bisplam gravels, described by Mr. Binney (1861) as containing nineteen species of shells now living in the Irish Sea, belong to this division. Shells are also abundant in it at Macclesfield.

The Upper Boulder-clay, or Till.—This member caps the sand over the flat ground extending from Stockport to Alderley. Amongst the hills of the Pennine Chain to the east, it frequently occupies the valleys, as at Broadbottom, New Mills, Chapel-en-le-Frith, and Saltersford. It occupies the districts of Haughton Green and Hyde, Denton, Newton, Fairfield, Failsworth, Hollinwood, Oldham, and the higher parts of Harpurhey and Blackley. It also forms a capping for the sand along the Irwell, from Pendlebury

House, by Clifton and Kearsley, to Bolton Moor; and, in a similar position, it occurs at Little Lever, Bradshaw, Harwood, and Elton, near Bury. Its general tendency is to form flat or gently rising surfaces, of a wet or marshy character; while the Middle Sand forms undulating banks, hillocks, and knolls, such as that of Tandle Hill, which reaches an elevation of 725 feet. Outliers of sand and gravel are also to be met with amongst the hills, as at Mossley, Lyme Park, and Bollington; and these may probably be referred to the same formation.

The succession of these Drift-deposits now described bears a remarkable resemblance to that exposed to view along the cliffs north of Blackpool, described by Mr. Binney*. But, although I am disposed to think they are the exact equivalents, it would be rash to pronounce an opinion on this point until a survey of the intermediate country has been completed.

The Position of the Drift-deposits with reference to the older Rocks now requires our attention; and in tracing the boundaries of these different divisions we become sensible of a universally pervading feature in their arrangement, namely, that they rise in the direction of the hills, or conversely slope from the hills towards the plains. This is true with regard to the high lands of millstone-grit which range from east to west, by Rochdale, Bury, and Bolton, as well as those which range from north to south, by Oldham, Staleybridge, Marple, and Macclesfield. This rise of the beds of Drift, both towards the north and towards the east, is more rapid than the slope of the brooks, until they actually enter the uplands, when the descent of the streams becomes in turn more rapid than that of the drift; and on this account the Lower Boulder-clay seldom extends into the valleys of the Pennine Chain, as already stated.

* Mem. Lit. & Phil. Society, vol. x. (new series).

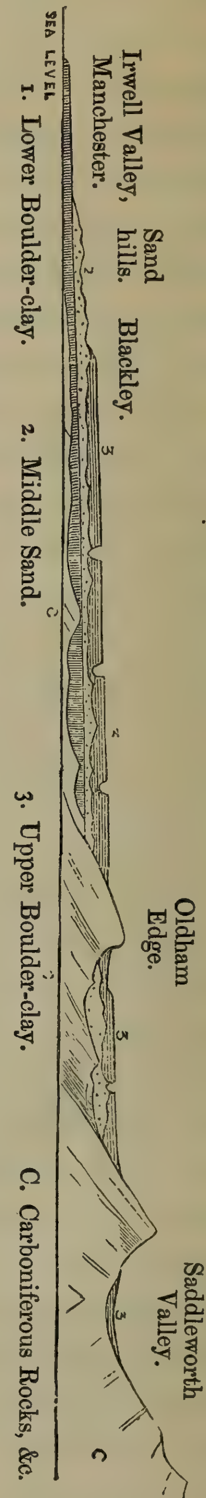
As an illustration, let us take the lower boundary of the Upper Boulder-clay along the valley which runs up from Manchester, by Bolton, to beyond Sharples, and examine the levels as taken from the Ordnance 6-inch maps. At Pendlebury the base of the Upper Boulder-clay is 275 feet above the sea-level; at Clifton, 285; at Kearsley, 300; at Halshaw Moor it descends again to 285; opposite Burnden Bridge it again reaches 300; at centre of Bolton, 300; Little Bolton, 370; the banks of the Tonge and Bradshaw brooks, near Bradshaw Bridge, 380; Sweetlove's Colliery, Sharples, 475; and still further north, at Holmes Farm, above Dunscar Bridge, 500 feet. Thus, in a distance of about nine miles along this valley, the base of the Upper Boulder-clay has ascended from 275 to 500 feet, that is, by an amount of 225 feet. The rise is therefore $\frac{1}{211\cdot2}$, or 25 feet per mile.

A similar rise is observable, if we take the section of country from Manchester to Oldham, or from Manchester to Dukinfield. Thus at Gorton the level of the base of the Upper Boulder-clay is 250 feet, and at Dukinfield (as may be determined at the sand-pit near St. John's Church) it is about 480 feet, being a rise of 230 feet in four miles. That there is a similar slope towards the valley of the Mersey from the Cheshire hills is proved by the position of the beds along the brook-courses, as already stated.

The different members of the Drift series rest indiscriminately on the older rocks, which were worn into hills and valleys, or plains, before their deposition (see fig. 3). Thus in Manchester the Lower Boulder-clay rests on the Triassic and Permian beds; but at Heywood, Rochdale, and Dukinfield the older rocks are covered by the Middle Sand; and all along the rising ground of the lower Coal-measures, from Oldham by Staleybridge, Marple, and Disley, the Upper Boulder-clay rests upon, or has been deposited against, the steeply sloping sides of the Carboniferous rocks.

In order to account for the phenomena above stated, regarding the slope of the Drift-formation from the hills toward the plains, and which bears a strong resemblance to a true dip of the strata, I at first supposed that it was due to an upheaval of the country at the close of the Drift period along the old lines of elevation; but Professor Ramsay suggested to me that a more simple explanation might be found in the unquestionable fact that these various beds of clay and sand were deposited over a sloping sea-bottom, and consequently partake of its variations of level. The height to which erratics ascend on these hills is about 1800 feet, as stated long since by Sir H. De la Beche; and from my own observation I can state that there is not a trace of a foreign rock on the tableland of the Peak, which is about 2000 feet high.

Fig. 3. *Section to show the General Arrangement of the Drift-deposits.*

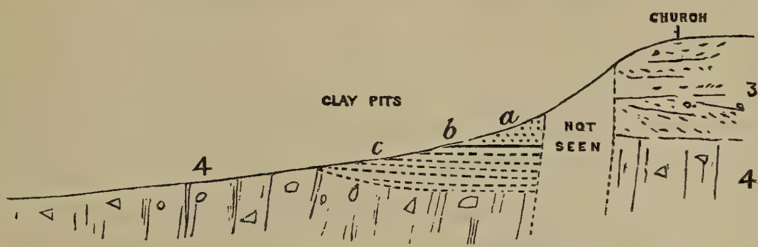


The following general section (fig. 3) will serve to explain the general phenomena connected with the relative position of the post-Pliocene and older formations in this district.

Supposed Land-surface in the Drift.—A very interesting section has been opened in certain beds which I am now about to describe, at the foot of the hill, west of Heaton Mersey. The hill itself is composed of the middle sand and gravel (No. 3); and along its base are large brick-yards excavated in the Lower Boulder-clay

(No. 4). At the upper edge of these brick-yards we find the following series, which, when I first visited it, I supposed to represent a land-surface in the Drift, forming the line of separation between the Lower Boulder-clay and the Middle Sand. The section is as follows:—

Fig. 4. *Section at Heaton Mersey.*



a. Fine soft sand, 3 feet.
b. Bed of peaty matter, and decaying stems and branches of birch, 4 inches.
c. Dark, stiff, laminated clay, 6 feet.
3. Middle Sand.
4. Lower Boulder-clay, with pebbles.

The bed of vegetable matter (*b*) consists of branches of birch in a state of decomposition not much removed from that of ordinary bog-wood. It is about 4 inches in thickness, is overlain by a bed of fine sand (*a*), which I supposed at first to be the base of the middle sand and gravel of which the hill is composed; and below are several feet of a fine, laminated, brownish mud (*c*), without pebbles, which I took to be the uppermost beds of the Lower Till. These beds, however, contain no stones or pebbles, as is usual with the Boulder Clay, and are more regularly laminated than is generally the case with that formation. At the same time, I had no reason to doubt that the whole series belonged to the post-Pliocene group, and that we had here a rare example of a true land-surface between two members thereof.

A few weeks after, however, I again visited the section in company with Professor Ramsay, F.R.S., the Director of the Geological Survey, who, on seeing the beds, gave it

as his opinion that these deposits were not post-Pliocene beds, but "warp," a river mud similar to that of the Humber, which he had recently visited. He also thought the stems of the birch too fresh-looking for so distant an age as the Drift, and that the deposit was an evidence of the former extension of the Mersey much beyond its present limits. We examined the bed for shells or any other objects calculated to throw light on the age of the beds, but without success; and, until further evidence of the extension or absence of the peat beneath the gravel of the Heaton Mersey hill, the question of the age of these beds must be left in abeyance. The section is 50 feet above the present level of the Mersey.

Gravel of the Valley of the Mersey.—The district of South Lancashire affords conclusive evidence of the former extension of the rivers far beyond their present bounds. The river-terraces in the neighbourhood of Manchester have already been described by Mr. Binney * and myself †, and I shall not recur to them here. I wish, however, to draw attention to an old terrace of much wider extent and greater length than any of those in the Irwell valley above Manchester ‡. So widely indeed is the country covered by these gravels, that it is not improbable they may have been formed in an estuary of the Dee, when the land was slowly rising from beneath the sea at the last elevation of the country; but on this point, which it would be of so much interest to determine, we are left in doubt by the absence of shells, which I have failed hitherto to detect.

The gravel is generally of a very fine character, evenly bedded, seldom containing large stones, and often divided by layers of fine sand and silt. On the north side of the Mersey it extends as far up as Didsbury, occupying the flat

* "On the Drift-deposits, &c.," Mem. Lit. and Phil. Soc. vol. viii.

† Memoir on the Geology of Bolton-le-Moors.

‡ This terrace I have described at greater length in the forthcoming memoir, "On the Geology of the Country around Oldham and Manchester."

ground along the Manchester road to Fallowfield. From this it trends westward to Hulme, on the south side of Manchester, and is bounded by the valley of the Irwell. It occupies the whole of the flat country between the two rivers, Irwell and Mersey, from Trafford Park to Stretford. At Eccles and Fatricroft it may be found resting sometimes on the New Red Sandstone, sometimes on the Lower Till, and it stretches westward by Barton Moss to Higher Irlam.

South of the Mersey it occupies the level plain, which is a constant subject of remark to all who travel by the railway to Altrincham; and the villages of Timperly, Sale, Ashton-on-Mersey, Carrington, and Warburton are all built on this old terrace. Beyond this I have not traced it westward. It probably disappears at Lymn, owing to the steepness of the banks along the south side of the river. On the north bank, however, it will probably be found between Hollinfare and Warrington. The thickness of this gravel is seldom more than from 6 to 10 feet; and over the greater part of the district described it rests upon the Lower Boulder-clay.

The breadth of this terrace in some places is several miles. As it extends very nearly from Worsley in the north to Altrincham in the south, the breadth is here seven miles. Below this terrace the present river-valleys are hollowed to a depth of 50 or 60 feet; and I have no doubt the land was lower at least by that amount at the time of its formation. The most probable explanation of the origin of this gravel-bed is to suppose that the tides extended as far up as Manchester and Didsbury, and that the waters of the two rivers, having only a very slight fall, often during heavy floods covered the whole plain now formed of the gravel.

XXXV. *A few Remarks on Mr. Hull's Additional Observations on the Drift-deposits in the Neighbourhood of Manchester.* By the President, E. W. Binney, F.R.S., F.G.S.

Read January 12th, 1864.

The author said he wished to make a few remarks on the Lancashire and Cheshire Drift. In the year 1841 he first attempted to class the Drift-deposits found in the neighbourhood of Manchester, in a small paper, with a map, which he prepared for the Statistical Society of Manchester. In that memoir he divided the foreign drift in the ascending order—

(1.) Lower sand and gravel,
(2.) Till,
(3.) Upper sand and gravel;

and he described the more modern deposits found in valleys (No. 4) as valley-gravel.

This order he adopted in a paper read before the Manchester Geological Society on the 22nd December 1842, "Notes on the Lancashire and Cheshire Drift," and printed by that Society in their Proceedings of 1843. In that paper, in treating of the upper beds of sand and gravel, he says, "At Manchester it (the Higher Drift) is composed of lower gravel, till, and sand and gravel, while at Heywood and Poynton, near the base of the Pennine Chain, the beds of sand and gravel are parted by several beds of loam and clay."

Again, in speaking of No. 3 deposit, he says, "The gently rising lands of the two counties are generally composed of this deposit. It varies much, both in its composition and thickness. Near the sea, at Ormskirk, the Till is sometimes found without it; but as you proceed to the east it makes its appearance, and gradually thickens until it attains its greatest thickness near the base of the Pennine

Chain. Not only does it increase in thickness, but it becomes more complex, and contains beds of clay, marl, and loam of several yards in thickness. The country lying between Manchester, Bolton, Bury, Rochdale, Ashton, and Stockport, for the most part, is upon it, and forms one great sandbank, which continues south into Cheshire."

The same classification he adopted in two papers, one on the Drift of Manchester, and the other on the same deposits at Blackpool, printed in vols. viii. and x. of the Society's Memoirs, as well as in a paper printed in the Manchester Geological Society's 'Transactions' for June 1862.

Mr. Hull, in his communication read at the last Meeting of the Society, divided the higher Drift-deposits into (in descending order)—

(1.) Upper Boulder-clay.
(2.) Middle Sand and Gravel.
(3.) Lower Boulder-clay.

The Nos. 2 and 3 had been described by Mr. Binney, as also a lower bed of sand and gravel, of whose existence he (Mr. Hull) had considerable doubts, and considered it as merely accidental.

Now in his (the author's) paper on the Drift of Manchester, 11 sections of wells and bores are given, and in 10 of these the lower sand and gravel had been met with, thus showing that it can scarcely be considered to be merely accidental as Mr. Hull states. In many other sections since examined in Lancashire this deposit has also been found under the Till. With regard to the upper bed of boulder-clay, Mr. Hull stated that he (the author) had alluded to it; but Mr. Hull considered it to be quite as important as the lower, both in thickness and area.

The old term "Till" is as good as that of Boulder-clay; and as it has been long used, there is not much use in changing it. During the last twenty years he had collected many

facts, which he intended to publish when he had completed his collection; but these did not show one bed of clay or marl which could be called Upper Boulder-clay, but several; in fact, there were numerous intercalations of it in the sand and gravel, one of which he had seen occurring at Kersall Moor, entirely surrounded by sand. To show the complexity of these deposits, and the difficulty of reducing them to two beds of Till or Boulder-clay, he gave two sections, one near Hyde and the other at Outwood*, where the following were met with:—

Hyde.

	feet	in.
Clay	11	0
Quicksand	2	6
Strong marl	22	6
Quicksand	2	6
Loam, with pebbles	12	6
Buckleaf marl	19	0
Dry sand	9	0
Quicksand and loam	6	0
Gravel	3	0
Loam	7	6
Gravel and sand	3	0
Clay and loam	15	6
Gravel and soft marl, containing pebbles	10	0
Coal-measures.		
	124	0

Outwood.

	feet	in.
Bog	11	0
Quicksand	53	3
Buckleaf marl	31	2
Red sand and gravel, with a yard of clay in it	15	0
Toad-back marl	32	3
Gravel	3	0
Coal-measures.		
	145	8

From the position of the Outwood section, in a slight depression, and the higher grounds adjoining being capped with a bed of clay containing pebbles, 8 or 10 feet in thickness, another deposit of clay should be placed on the top. Thus in one case there are 6 beds of Boulder-clay, and in the other only 3. These are two of the many instances which could be adduced, and suggest caution in attempting to classify these deposits without collecting and consulting numerous sections.

* For these the author was indebted to the kindness of Mr. Joseph Goodwin, mining engineer, Hyde and Haughton Collieries.

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1854, Jan. 24. Morin, Arthur, Gr. Off. Legion of Honour, General of Brigade, Mem. Imper. Instit. France, formerly élève Polytechn. School, Dir. Conserv. of Arts, Paris, Corr. Mem. Roy. Acadd. Sc. Berlin, Madrid and Turin, Acad. Georg. Florence, Imper. Acad. Metz, and Industr. Soc. Mulhouse. 3 *Rue des Beaux-Arts, Paris.*

1843, Apr. 18. Moseley, Rev. Henry, M.A., F.R.S., Corr. Mem. Imper. Instit. France. *Olveston, near Bristol.*

1821, Jan. 26. Mosley, Sir Oswald, Bart., D.C.L. *Rolleston Hall, Burton-on-Trent.*

1844, Apr. 30. Murchison, Sir Roderick Impey, G.C.St.S., D.C.L., M.A., F.R.S., F.G.S., F.L.S., &c., Director Gen. of the Geol. Survey, Pr. R.G.S., Hon. Mem. R.S.E. and R.I.A., Mem. C.P.S. and Imper. Acad. Sc. Petersburg, Corr. Mem. Imper. Instit. France, Roy. Acadd. Sc. Stockholm, Turin, Berlin and Brussels, Roy. Soc. Sc. Copenhagen, Amer. Acad. Arts and Sc. Boston, and Imper. Geogr. Soc. Petersburg, Hon. Mem. Imper. Soc. of Naturalists Moscow, &c. 16 *Belgrave-square, London,* S.W.

1844. Apr. 30. Owen, Richard, M.D., LL.D., F.R.S., F.L.S., F.G.S., V.P.Z.S., Director of the Nat. Hist. Department British Museum, Hon. F.R.C.S. Ireland, Hon. M.R.S.E., For. Assoc. Imper. Instit. France, Mem. Imper. Acadd. Sc. Vienna and Petersburg, Imper. Soc. of Naturalists Moscow, Roy. Acadd. Sc. Berlin, Turin, Madrid, Stockholm, Munich, Amsterdam, Naples, Brussels and Bologna, Roy. Socc. Sc. Copenhagen and Upsala, and Amer. Acad. Arts and Sc. Boston, Corr. Mem. Philom. Soc. Paris, Mem. Acad. Georg. Florence, Soc. Sc. Haarlem and Utrecht, Soc. of Phys. and Nat. Hist. Geneva, Acad. dei Nuovi Lincei Rome, Roy. Acadd. Sc. Padua, Palermo, Acad. Gioen. Catania, Phys. Soc.

DATE OF ELECTION.

Berlin, Chev. of the Prussian Order "*Pour le Mérite*," For. Assoc. Instit. Wetter., Philadelphia, New York, Boston, Imper. Acad. Med. Paris, and Imper. and Roy. Med. Soc. Vienna. *British Museum, London*, W.C.

1851, Apr. 29. Playfair, Lyon, C.B., Ph.D., F.R.S., F.G.S., F.C.S., Professor of Chemistry Univ. Ed. *Edinburgh.*

1856, Jan. 22. Poncelet, General Jean Victor, For. Mem. R.S., Gr. Off. Legion of Honour, Mem. Imper. Instit. France, &c. 58 *Rue de Vaugirard, Paris.*

1859, Apr. 19. Rankine, William John Macquorn, LL.D., F.R.SS. L. and E., Pres. Inst. Eng. Scot., Regius Professor of Civil Engineering and Mechanics Univ. Glasgow. 59 *St. Vincent-street, Glasgow.*

1849, Jan. 23. Rawson, Robert. *Royal Dockyard, Portsmouth.*

1859, Apr. 19. Reichenbach, Carl, Baron von. *Gut Reissenberg, nächst Grinzing, Vienna.*

1844, Apr. 30. Sabine, Major-General Edward, R.A., D.C.L., Treas. and P.R.S., F.R.A.S., Hon. Mem. C.P.S., Chev. of the Prussian Order "*Pour le Mérite*," Mem. Imper. Acad. Sc. Petersburg, Roy. Acadd. Sc. Berlin, Brussels, and Gottingen, Roy. Soc. Sc. Drontheim, Acad. Sc. Philadelphia, Econ. Soc. Silesia, Nat. Hist. Soc. Lausanne, and Roy. Batavian Soc., Corr. Mem. Roy. Acad. Sc. Turin, Nat. Instit. Washington, U.S., Geogr. Socc. Paris, Berlin, and Petersburg. 13 *Ashley-place, Westminster, London*, S.W.

1843, Feb. 7. Sedgwick, Rev. Adam, M.A., F.R.S., Hon. M.R.I.A., F.G.S., F.R.A.S., Woodwardian Lecturer Univ. Cambridge. *Trinity College, Cambridge.*

1851, Apr. 29. Stokes, George Gabriel, M.A., D.C.L., Secr. R.S., Lucasian Professor of Mathem. Univ. Cambridge, F.C.P.S., Mem. Batav. Soc. Rotterdam, Corr. Mem. Roy. Acad. Sc. Berlin. *Pembroke College, Cambridge.*

1861, Jan. 22. Sylvester, James Joseph, M.A., F.R.S., Professor of Mathematics. *Royal Military Academy, Woolwich, London*, S.E.

1854, Jan. 24. Tayler, Rev. John James, B.A., Principal of Manchester New College. *The Lymes, Rosslyn, Hampstead, London.*

DATE OF ELECTION.

1851, Apr. 29. Thomson, William, M.A., LL.D., F.R.SS. L. and E., Prof. of Nat. Philos. Univ. Glasgow. 2 *College, Glasgow.*

1843, Feb. 7. Whewell, Rev. William, D.D., F.R.S., Hon. M.R.I.A., F.S.A., F.G.S., F.R.A.S., Master of Trinity College Cambridge. *The Lodge, Cambridge.*

1850, Apr. 30. Woodcroft, Bennet, F.R.S., Professor, Superint. of Regist. of Patents. *Southampton-buildings, London,* W.C.

CORRESPONDING MEMBERS.

1860, Apr. 17. Ainsworth, Thomas. *Cleator Mills, near Egremont, Whitehaven.*

1861, Oct. 29. Bache, Alexander Dallas, For. Mem. R.S., Superintend. of the U.S. Coast Survey. *Washington, U.S.*

1861, Jan. 22. Buckland, George, Professor, University College, Toronto. *Toronto.*

1824, Jan. 23. Dockray, Benjamin. *Lancaster.*

1861, Apr. 2. Durand-Fardel, Max, M.D., Chev. of the Legion of Honour, &c. 36 *Rue de Lille, Paris.*

1849, Apr. 17. Girardin, J., Off. Legion of Honour, Corr. Mem. Imper. Instit. France, &c. *Lille.*

1862, Jan. 7. Gistel, Johannes Franz Xavier, Ph.D., late Prof. of Nat. Hist. and Geogr., Libr. Secr. and Conserv. at the Museum of Nat. Hist. Regensburg, Corr. Mem. Imper. Roy. Geol. Inst. Vienna, Acadd. and Socc. Sc. Cherbourg, Caen, Dijon, Aix, Orleans, Angers, Brussels, Rheims, Nantes, Antwerp, Linnean Socc. Caen, Angers, Marseilles, La Rochelle and Paris. 19 *Steinweg, Regensburg, Bavaria.*

1812, Jan. 24. Granville, Augustus Bozzi, M.D., F.R.S., V.P.O.S., M.R.C.P. Lond., M.R.C.S. Engl., Knt. of the Order of St. Michael of Bavaria, of the Crown of Würtemberg, of the Lion of Züringen of Baden, and of St. Maurice and St. Lazarus of Sardinia, For. Mem.

DATE OF ELECTION.

Imper. Acad. Sc. Petersburg, Roy. Acadd. Sc. Turin and Naples, Nat. Hist. Soc. Dresden, Philom. Soc., Soc. Méd. d'Emulat. and Cercle Méd. Paris, Socc. Georg. and Curéo, Florence, Med.-Chir. Socc. Petersburg and Berlin, Corr. Mem. Roy. Acad. Sc. Brussels, &c. 5 *Cornwall-terrace, Warwick-square, London,* S.W.

1850, Apr. 30. Harley, Rev. Robert, F.R.A.S. *Castle Hill House, Brighouse, Yorkshire.*

1861, Jan. 22. Henry, Joseph, Professor, Secr. Smithsonian Institution. *Washington, U.S.*

1812, Jan. 24. Holland, Sir Henry, Bart., M.D., D.C.L., LL.D., F.R.S., F.R.C.P. Lond., F.G.S., Physician in Ordinary to the Queen. 25 *Brook-street, London,* W.

1816, Apr. 26. Kenrick, Rev. John, M.A. *York.*

1838, Apr. 17. Koechlin-Schouch, Daniel. *Mulhouse.*

1862, Jan. 7. Lancia di Brolo, Federico, Inspector of Studies, &c. *Palermo.*

1859, Jan. 25. Le Jolis, Auguste-François, Ph.D., Archiviste perpétuel and late President of the Imper. Soc. Nat. Sc. Cherbourg, Mem. Imp. Leop.-Car. Acad. Nat. Sc., Imp. Soc. Naturalists Moscow, Acad. Nat. Sc. Philadelphia, Roy. Botan. Socc. Regensburg, Leiden, Edinburgh, Botan. Soc. Canada, Linnean Socc. Lyon, Bordeaux, and Caen, Physiogr. Soc. Lund, Imp. Roy. Geol. Instit. Vienna, Imp. Roy. Zool. and Botan. Soc. Vienna, Roy. Acad. Sc. Lucca and Prague, Imp. Acad. Sc. and Lit. Chambery, Toulouse, Rouen, Caen, Lille, &c., Acad. Socc. Cherbourg and Angers, Hortic. Soc. Cherbourg, Roy. Acad. Archeol. Brussels, Socc. Nat. Sc. Catania, Athens, Boston, Dorpat, Riga, &c. *Cherbourg.*

1857, Jan. 27. Lowe, Edward Joseph, F.R.A.S., F.G.S., Mem. Brit. Met. Soc., Hon. Mem. Dublin Nat. Hist. Soc., Mem. Geol. Soc. Edinburgh, &c. *Nottingham.*

1861, Oct. 29. Maury, Captain Mathew Fontaine, LL.D., &c.

1864, Apr. 19. Mitchell, Captain John, Superintendent of the Madras Museum. *Madras.*

1862, Jan. 7. Nasmyth, James, C.E., F.R.A.S., &c. *Penshurst, Tunbridge.*

DATE OF ELECTION.

1851, Apr. 29. Pincoffs, Peter, M.D., Knt. of the Turkish Order of the "*Medjidié*" 4th Cl., Mem. Coll. Phys. London, Brussels, and Dresden, Hon. and Corr. Mem. Med. and Phil. Socc. Antwerp, Athens, Brussels, Constantinople, Dresden, Rotterdam, Vienna, &c. *Naples.*

1808, Nov. 18. Roget, Peter Mark, M.D., F.R.S., F.R.C.P. Lond., F.G.S., F.R.A.S., V.P.S.A., Corr. Mem. Roy. Acad. Sc. Turin. 18 *Upper Bedford-place, London,* W.C.

1834, Jan. 24. Watson, Henry Hough. *Bolton, Lancashire.*

1853, Apr. 19. Wilkinson, Thomas Turner, F.R.A.S. *Burnley.*

ORDINARY MEMBERS.

1839, Apr. 30. Ainsworth, Ralph Fawsett, M.D., F.R.C.P. Edin., M.R.C.S. Engl., F.R. Med. Chir. S. *Cliff Point, Lower Broughton, and Union Club, Mosley-street.*

1861, Jan. 22. Alcock, Thomas, M.D., Extr. L.R.C.P. Lond., M.R.C.S. Engl., L.S.A. 66 *Upper Brook-street.*

1861, Jan. 22. Anson, Rev. George Henry Greville, M.A. *Birch Rectory, Rusholme.*

1837, Aug. 11. Ashton, Thomas. 42 *Portland-street.*

1846, Jan. 27. Atkinson, John, F.C.P., F.G.S. *Thelwall, near Warrington.*

1824, Jan. 23. Barbour, Robert. 18 *Aytoun-street.*

1840, Jan. 21. Bateman, John Frederick, F.R.S., M.Inst.C.E., F.G.S. 16 *Great George-street, Westminster, London,* S.W.

1858, Jan. 26. Baxendell, Joseph, F.R.A.S., Corr. Mem. Roy. Phys. Econ. Soc. Konigsberg, and Ac. Sc. and Lit. Palermo. 108 *Stocks-street.*

1847, Jan. 26. Bazley, Thomas, M.P. *Eynsham Hall, Oxford.*

1847, Jan. 26. Bell, William. 51 *King-street.*

1858, Jan. 26. Benson, Davis. 4 *Chester-street.*

1854, Jan. 24. Beyer, Charles. 9 *Hyde-road, Ardwick.*

1842, Jan. 25. Binney, Edward William, F.R.S., F.G.S. 40 *Cross-street.*

1821, Jan. 26. Blackwall, John, F.L.S. *Hendre, Llanrwst.*

1861, Jan. 22. Bottomley, James. 2 *Nelson-street, Lower Broughton.*

DATE OF ELECTION.

1855, Jan. 23. Bowman, Eddowes, M.A. *Upper Park-road, Victoria Park.*

1839, Oct. 29. Bowman, Henry. *Upper Park-road, Victoria Park.*

1855, Apr. 17. Brockbank, William. 37 *Princess-street.*

1861, Apr. 2. Brogden, Henry. *Brooklands, near Sale.*

1844, Jan. 23. Brooks, William Cunliffe, M.A. *Bank,* 92 *King-street.*

1860, Jan. 24. Brothers, Alfred. 14 *St. Ann's-square.*

1846, Jan. 27. Browne, Henry, M.D., M.A., M.R.C.S. Engl. 206 *Oxford-street.*

1861, Jan. 22. Buckley, Rev. Thomas, M.A. *Balmoral-place, Old Trafford.*

1847, Jan. 26. Calvert, Frederick Crace, Ph.D., F.R.S., F.C.S., Corr. Mem. Roy. Acad. Sc. Turin, Acad. Sc. Rouen, Pharmac. Soc. Paris, and Industr. Soc. Mulhouse. *Royal Institution, Bond-street.*

1859, Jan. 25. Carrick, Thomas. 37 *Princess-street.*

1858, Jan. 26. Casartelli, Joseph. 43 *Market-street.*

1852, Apr. 20. Chadwick, David, F.S.S., Assoc. Inst. C.E. 75 *King-street.*

1842, Jan. 25. Charlewood, Henry. 5 *Clarence-street.*

1857, Apr. 21. Churchill, George Cheetham. 86 *Cross-street.*

1854, Apr. 18. Christie, Richard Copley, M.A., Prof. Hist. Owens College. 7 *St. James's-square.*

1862, Feb. 18. Clarke, Thomas, M.D. *Ladyfield, Wilmslow.*

1841, Apr. 20. Clay, Charles, M.D., Extr. L.R.C.P. Lond., L.R.C.S. Edin. 101 *Piccadilly.*

1861, Jan. 22. Clifton, Robert Bellamy, M.A., F.R.A.S., Prof. Nat. Phil. Owens College. *Owens College.*

1853, Jan. 25. Cottam, Samuel. 28 *Brazenose-street.*

1859, Jan. 25. Coward, Edward. *Heaton Mersey, near Manchester.*

1861, Nov. 12. Coward, Thomas. *Bowdon.*

1851, Apr. 29. Crompton, Samuel, M.R.C.S. Engl., L.S.A., F.R. Med. Chir. Soc. 79 *Princess-street.*

1848, Jan. 25. Crowther, Joseph Stretch. 22 *Princess-street.*

1861, Apr. 2. Cunningham, William Alexander. *Bank,* 37 *King-street.*

1854, Feb. 7. Dale, John, F.C.S. *Cornbrook Chemical Works, Chester-road.*

1842, Apr. 19. Dancer, John Benjamin, F.R.A.S. 43 *Cross-street.*

1863, Feb. 10. Darbishire, George Stanley. 32 *Charlotte-street.*

1853, Apr. 19. Darbishire, Robert Dukinfield, B.A., F.G.S. 21 *Brown-street.*

DATE OF ELECTION.

1854, Jan. 24. Davies, David Reynold. 33 *Dickinson-street.*
1842, Nov. 15. Dean, James Joseph. 2 *Grove-street, Ardwick.*
1861, Dec. 10. Deane, William King. 25 *George-street.*
1855, Jan. 23. Dickinson, William Leeson. 1 *St. James's-street.*
1859, Jan. 25. Dorrington, James. 33 *Dickinson-street.*
1864, Mar. 22. Duval, C. A. *Exchange-street.*
1818, Apr. 24. Dyer, Joseph Chesborough. *Burnage.*

1859, Jan. 25. Eadson, Richard. 75 *Dale-street.*
1864, Apr. 5. Eastham, John. *St. Ann's-square.*
1856, Apr. 29. Ekman, Charles Frederick. 41 *George-street.*
1854, Jan. 24. Ellis, Charles. 21 *Rook-street, York-street.*

1824, Oct. 29. Fairbairn, William, C.E., LL.D., F.R.S., F.G.S., Corr. Mem. Imp. Inst. France and Roy. Acad. Sc. Turin, Hon. Mem. Inst. Eng. Scot. and Yorksh. Phil. Soc. *Polygon, Ardwick.*
1861, Jan. 22. Fisher, William Henry. 16 *Tib-lane.*
1856, Apr. 29. Forrest, Henry Robert. *Portland-street.*
1857, Apr. 21. Foster, Thomas Barham. 23 *John Dalton-street.*
1860, Apr. 17. Francis, John. *Town Hall.*
1854, Jan. 24. Fryer, Alfred. 4 *Chester-street.*

1840, Jan. 21. Gaskell, Rev. William, M.A. 46 *Plymouth-grove.*
1861, Apr. 30. Gladstone, Murray, F.R.A.S. 24 *Cross-street.*
1817, Jan. 24. Greg, Robert Hyde, F.G.S. 2 *Chancery-place, Booth-street.*
1849, Oct. 30. Greg, Robert Philips, F.G.S. 2 *Chancery-place, Booth-street.*
1863, Apr. 21. Grindon, Leopold Hartley. 85 *Rumford-street.*
1848, Jan. 25. Grundy, John Clowes. 4 *Exchange-street.*

1844, Jan. 23. Hampson, Richard. *Withington.*
1864, Feb. 9. Harris, George. *Cornbrook Park.*
1858, Oct. 19. Harrison, William Philip, M.D. *Ilkley Wells House, near Otley, Yorkshire.*
1862, Nov. 4. Hart, Peter. 45 *Back George-street.*
1839, Jan. 22. Hawkshaw, John, F.R.S., F.G.S., M.Inst. C.E. 33 *Great George-street, Westminster, London,* S.W.
1861, Apr. 2. Haywood, George Robert. 1 *Newall's Buildings, Market-street.*
1859, Apr. 19. Heelis, Thomas, F.R.A.S. 75 *Princess-street.*
1828, Oct. 31. Henry, William Charles, M.D., F.R.S. 11 *East-street, Lower Mosley-street.*
1861, Apr. 30. Heys, William Henry. *Hazel-grove, near Stockport.*

DATE OF ELECTION.

1815, Jan. 27. Heywood, Sir Benjamin, Bart., F.R.S. *Claremont, near Manchester.*

1833, Apr. 26. Heywood, James, F.R.S., F.G.S., F.S.A. 26 *Kensington Palace Gardens, London,* W.

1864, Mar. 22. Heywood, Oliver. *Bank, St. Ann's-street.*

1851, Apr. 29. Higgin, James. *Hulme Hall Chemical Works, Chester-road.*

1845, Apr. 29. Higgins, James. *King-street, Salford.*

1848, Oct. 31. Higson, Peter. 94 *Cross-street.*

1839, Jan. 22. Hobson, John. *Bakewell, Derbyshire.*

1861, Apr. 2. Hobson, John Thomas, Ph.D. *Alton-terrace, Gildabrook.*

1854, Jan. 24. Holcroft, George. 5 *Red Lion-street, St. Ann's-square.*

1855, Jan. 23. Holden, Isaac. 64 *Cross-street.*

1846, Jan. 27. Holden, James Platt. *St. James's Chambers,* 3 *South King-street.*

1823, Apr. 18. Hopkins, Thomas, M. Brit. Met. Soc. 35 *Broughton-lane.*

1824, Jan. 23. Houldsworth, Henry. *Newton-street Mills,* 34 *Little Lever-street.*

1863, Nov. 3. Hull, Edward, B.A., F.G.S. 34 *Windsor-place, Cheetham.*

1857, Jan. 27. Hunt, Edward, B.A., F.C.S. 20 *Devonshire-street, All Saints.*

1859, Jan. 25. Hurst, Henry Alexander. 61 *George-street.*

1850, Apr. 30. Johnson, Richard, F.C.S. *Oak Bank, Fallowfield.*

1821, Oct. 19. Jordan, Joseph, F.R.C.S. Engl. 70 *Bridge-street.*

1848, Apr. 18. Joule, Benjamin St. John Baptist. *Thorncliff, Old Trafford.*

1842, Jan. 25. Joule, James Prescot, LL.D., F.R.S., F.C.S., Hon. Mem. C.P.S., and Inst. Eng. Scot., Corr. Mem. Roy. Acad. Sc. Turin. *Thorncliff, Old Trafford.*

1843, Jan. 24. Kay, Samuel. 6 *Fountain-street.*

1852, Jan. 27. Kennedy, John Lawson. 47 *Mosley-street.*

1862, Apr. 29. Knowles, Andrew. *High-bank, Pendlebury.*

1830, Apr. 30. Langton, William. *Manchester and Salford Bank, Mosley-street.*

1860, Jan. 24. Latham, Arthur George. 24 *Cross-street.*

1863, Dec. 15. Leake, Robert. 100 *Mosley-street.*

1850, Apr. 30. Leese, Joseph. *Altrincham.*

1860, Jan. 24. Leigh, John, M.R.C.S. Engl., L.S.A., F.C.S. 26 *St. John's-street.*

DATE OF ELECTION.

1839, Oct. 29. Lockett, Joseph. 100 *Mosley-street.*

1857, Jan. 27. Longridge, Robert Bentink. 1 *New Brown-street.*

1854, Jan. 24. Lowe, George Cliffe. 26 *St. Ann's-street.*

1850, Apr. 30. Lund, Edward, M.R.C.S. Engl., L.S.A. 22 *St. John's-street.*

1855, Jan. 23. Lund, George Taylor. 5 *Southgate, St. Mary's.*

1859, Jan. 25. Lynde, James Gascoigne, M.Inst.C.E., F.G.S. *Town Hall.*

1855, Oct. 30. Mabley, William Tudor. 14 *St. Ann's-square.*

1829, Oct. 30. McConnel, James. *Bent-hill, Prestwich.*

1838, Apr. 17. McConnel, William. 90 *Henry-street, Oldham-road.*

1844, Apr. 30. McDougall, Alexander. 11 *Riga-street, Hanover-street.*

1823, Jan. 24. Macfarlane, John. *Edge-hill House, Coney-hill, Bridge of Allan, Scotland.*

1859, Jan. 25. Maclure, John William, F.R.G.S. 2 *Bond-street.*

1849, Apr. 17. Manchester, The Right Rev. the Lord Bishop of, D.D., F.R.S., F.G.S., F.C.P.S., Corr. Mem. Arch. Inst. Rome. *Diocesan Registry Office,* 7 *St. James's-square.*

1858, Apr. 20. Mather, Colin. *Iron Works, Deal-street, Brown-street, Salford.*

1842, Jan. 25. Mellor, Thomas. 204 *Oxford-street.*

1837, Jan. 27. Mellor, William. *Lime Works, Ardwick.*

1864, Mar. 8. Micholls, Horatio. *Nicholas-street.*

1864, Mar. 22. Montefiore, Leslie J., 17 *Cannon-street.*

1861, Oct. 29. Morgan, John Edward, M.B., M.A., M.R.C.P. Lond., F.R.Med. and Chir.S. 33 *King-street.*

1849, Jan. 23. Morris, David. 1 *Market-place.*

1864, Mar. 22. Mudd, James. *St. Ann's-square.*

1822, Apr. 26. Neild, William. *Mayfield Print Works, Buxton-street.*

1852, Jan. 27. Nelson, James Emanuel. 17 *Bridgewater-place, High-street.*

1854, Feb. 7. Nevill, Thomas Henry. 19 *George-street.*

1860, Jan. 24. Newall, Henry. *Hare-hill, Littleborough.*

1862, Dec. 30. Ogden, Samuel. 10 *Back Mosley-street.*

1861, Jan. 22. O'Neill, Charles, F.C.S., Corr. Mem. Industr. Soc. Mulhouse. 4 *Bank-place, St. Phillip's Church, Salford.*

1844, Apr. 30. Ormerod, Henry Mere. 5 *Clarence-street.*

1861, Apr. 30. Parlane, James. 10 *Dickinson-street.*

DATE OF ELECTION.

1861, Jan. 22.	Parr, George, jun. *Phœnix Works, Chapel-street, Ancoats.*
1833, Apr. 26.	Parry, John. 100 *Mosley-street.*
1861, Jan. 22.	Perring, John Shae, M.Inst.C.E. 104 *King-street.*
1861, Jan. 22.	Pincoffs, Simon. 57 *George-street.*
1857, Apr. 21.	Platt, William Wilkinson. *Iron Works, Deal-street, Brown-street, Salford.*
1854, Jan. 24.	Pochin, Henry Davis. 42 *Quay-street, Salford.*
1860, Apr. 17.	Pocklington, Rev. Joseph Nelsey, B.A. 203 *York-street, Hulme.*
1861, Jan. 22.	Preston, Francis. *Ancoats Bridge Works, Limekiln-lane, Ardwick.*
1861, Jan. 22.	Radford, William. 41 *John Dalton-street.*
1854, Feb. 7.	Ramsbottom, John. *Railway Station, Crewe.*
1859, Apr. 19.	Ransome, Arthur, B.A., M.B. Cantab., M.R.C.S. 1 *St. Peter's-square.*
1859, Jan. 25.	Rideout, William Jackson. 11 *Church-street.*
1860, Jan. 24.	Roberts, William, M.D., B.A., M.R.C.P. Lond. 10 *Chatham-street, Piccadilly.*
1822, Jan. 25.	Robinson, Samuel. *Black Brook Cottage, Wilmslow.*
1864, Jan. 12.	Rogerson, John. *Gaythorn.*
1858, Jan. 26.	Roscoe, Henry Enfield, B.A., Ph.D., F.R.S., F.C.S., Professor of Chemistry, Owens College. *Owens College.*
1851, Apr. 29.	Sandeman, Archibald, M.A., Professor of Mathematics, Owens College. *Owens College.*
1842, Jan. 25.	Schunck, Edward, Ph.D., F.R.S., F.C.S. *Oaklands, Kersal.*
1863, Apr. 7.	Schwabe, Edmund Salis, B.A., F. Anthrop. Soc. 41 *George-street.*
1858, Oct. 19.	Sever, Charles. *Palatine-buildings.*
1855, Jan. 23.	Sharp, Edmund Hamilton. *Seymour-grove, Old Trafford.*
1835, Oct. 30.	Shuttleworth, John. *Wilton Polygon, Cheetham-hill.*
1852, Apr. 20.	Sidebotham, Joseph. 19 *George-street.*
1859, Jan. 25.	Slagg, John, jun. 12 *Pall Mall.*
1838, Jan. 26.	Smith, George Samuel Fereday, M.A., F.G.S. 2 *Essex-street, King-street.*
1845, Apr. 29.	Smith, Robert Angus, Ph.D., F.R.S., F.C.S., Corr. Mem. I.R. Geol. Inst. Vienna. 20 *Devonshire-street, All Saints.*
1859, Jan. 25.	Sowler, Thomas. 4 *St. Ann's-square.*

DATE OF ELECTION.

1851, Apr. 29.	Spence, Peter, F.C.S., M.S.A. *Alum Works, Newton-heath.*
1852, Jan. 27.	Standring, Thomas. 1 *Piccadilly.*
1847, Apr. 20.	Stephens, James, F.R.C.S., L.S.A. 68 *Bridge-street.*
1858, Jan. 26.	Stewart, Charles Patrick. *Atlas Works,* 88 *Great Bridgewater-street, and Oaklands, Victoria-park.*
1863, Oct. 6.	Stretton, Bartholomew. *Bridgewater-place, High-street.*
1814, Jan. 21.	Stuart, Robert. *Ardwick Hall.*
1859, Jan. 25.	Tait, Mortimer Lavater. 95 *St. James's-street.*
1856, Jan. 22.	Taylor, John Edward. 3 *Cross-street.*
1860, Apr. 17.	Trapp, Samuel Clement. 18 *Cooper-street.*
1836, Apr. 29.	Turner, James Aspinall, M.P. 50 *Cross-street.*
1821, Apr. 19.	Turner, Thomas, F.R.C.S. Engl., F.L.S., F.R. Med. Chir. S., Hon. F. Harv. Soc. 77 *Mosley-street.*
1861, Apr. 30.	Vernon, George Venables, F.R.A.S., F. Anthrop. Soc., Mem. Brit. Met. Soc., Met. Soc. Scotl., and Met. Soc. France. *Auburn-street, Piccadilly.*
1857, Jan. 27.	Walker, Robert, M.D., L.R.C.S. Edin. 89 *Mosley-street.*
1859, Jan. 25.	Watson, John. *Rose-hill, Bowdon.*
1857, Jan. 27.	Webb, Thomas George. *Glass Works, Kirby-street, Ancoats.*
1861, Oct. 15.	Whalley, John. 14 *Marsden-street.*
1858, Jan. 26.	Whitehead, James, M.D., M.R.C.P. Lond., F.R.C.S. Engl., L.S.A., M.R.I.A., Corr. Mem. Soc. Nat. Phil. Dresden, Med. Chir. Soc. Zurich and Obst. Soc. Edin., Mem. Obst. Soc. Lond. 87 *Mosley-street.*
1839, Jan. 22.	Whitworth, Joseph, F.R.S. *Chorlton-street, Portland-street.*
1859, Jan. 25.	Wilde, Henry. 2 *St. Ann's-churchyard.*
1859, Apr. 19.	Wilkinson, Thomas Read. *Manchester and Salford Bank, Mosley-street.*
1853, Apr. 19.	Williamson, Samuel Walker. *St. Mark's-place, Cheetham-hill.*
1851, Apr. 29.	Williamson, William Crawford, F.R.S., Professor of Natural History, Anat. and Physiol. Owens College, M.R.C.S. Engl., L.S.A. 172 *Egerton-road, Fallowfield.*
1864, Mar. 8.	Windsor, Thomas, M.R.C.S. 65 *Piccadilly.*
1851, Jan. 21.	Withington, George Bancroft. 24 *Brown-street.*

DATE OF ELECTION.	
1836, Jan. 22.	Wood, William Rayner. *Singleton Lodge, near Manchester.*
1855, Oct. 30.	Woodcock, Alonzo Buonaparte. *Orchard Bank, Altrincham.*
1860, Apr. 17.	Woodcroft, Rufus Dewar. *Cornbrook Chemical Works, Chester-road.*
1860, Apr. 17.	Woolley, George Stephen. 69 *Market-street.*
1840, Apr. 28.	Worthington, Robert, F.R.A.S. 96 *King-street.*
1863, Nov. 17.	Worthington, Samuel Barton, C.E. *Crescent-road, Cheetham-hill.*

NOTE.—*It is requested that any mistakes or alterations in the designations or addresses of Members, as given in this list, be notified to the Secretaries of the Society.*

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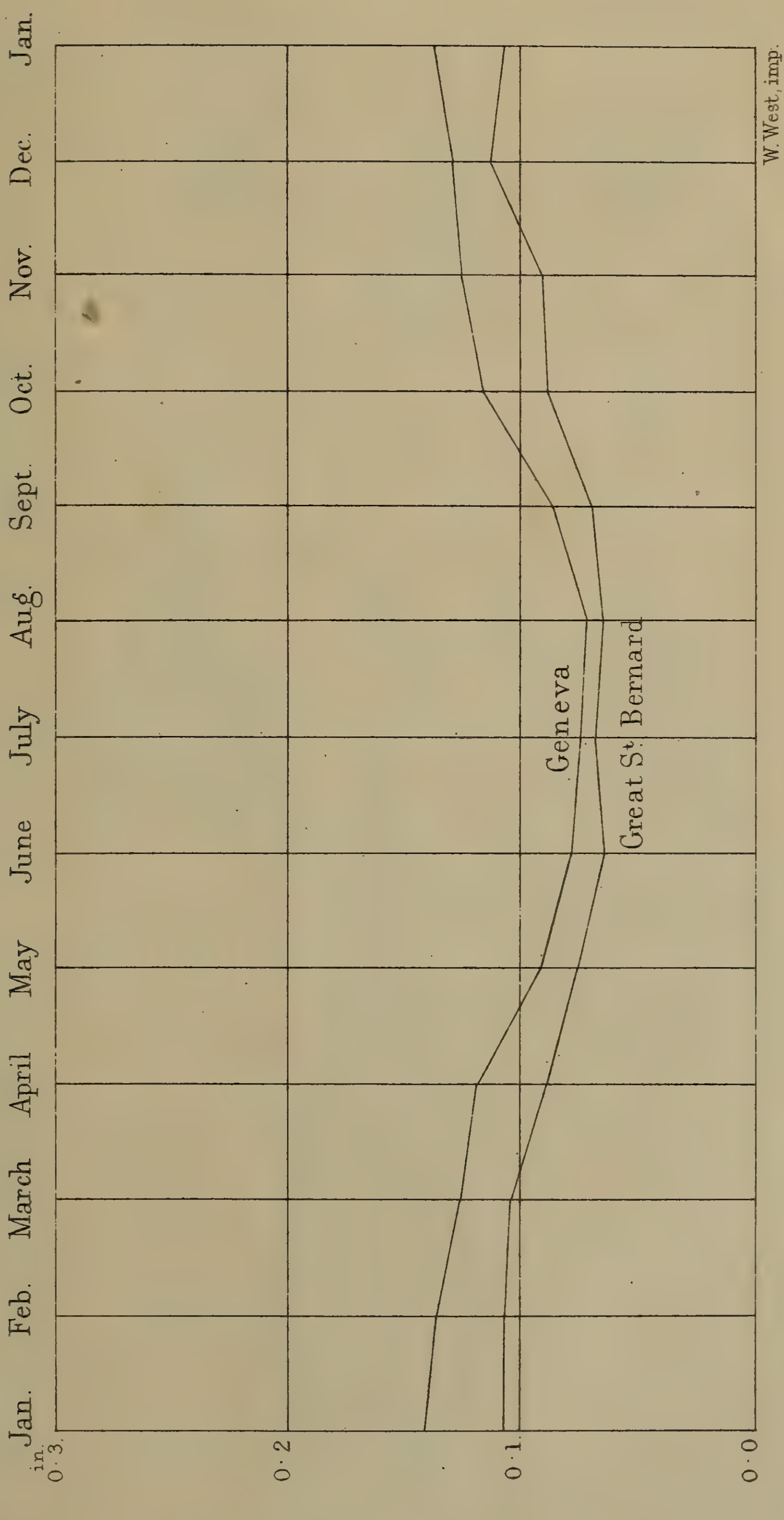
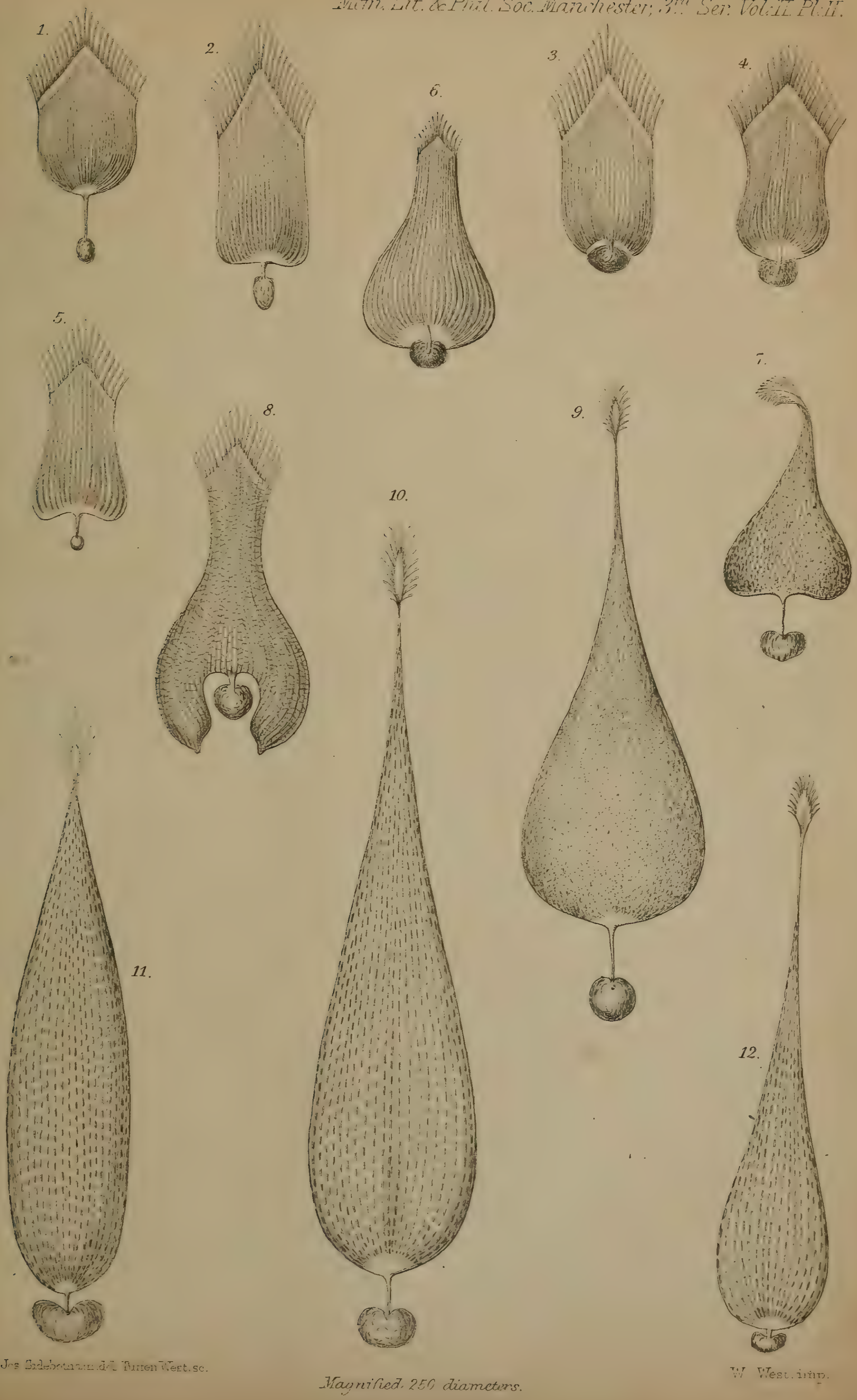


Monthly mean daily Amount of the Irregular Oscillations of the Barometer at Geneva and on the Great St. Bernard.



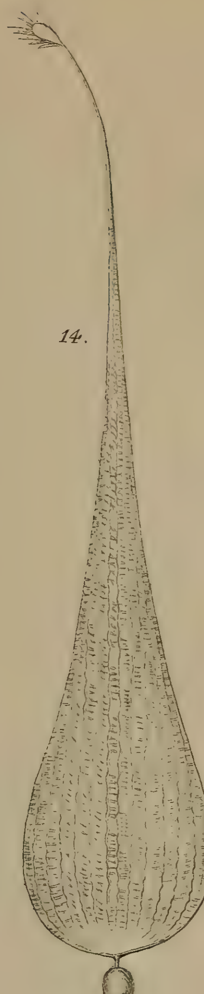
Jos Sidebotham del. Tuffen West sc.

Magnified 250 diameters.

W. West imp.

13.


15.


14.


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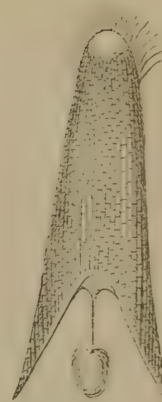

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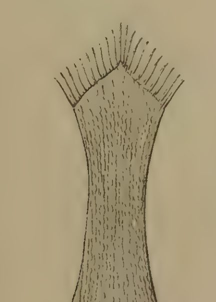

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24.


Jos. Sidebotham, del. Tuffen West, sc.

Magnified 250 diameters.

W. West, imp.

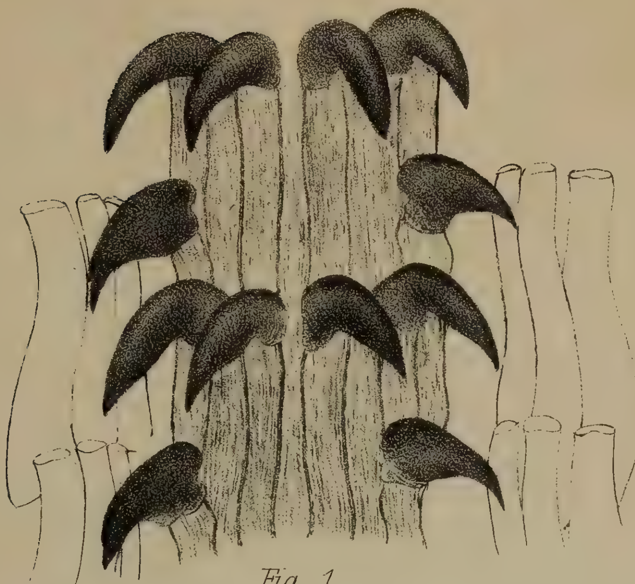

Fig. 1.

Fig. 3.

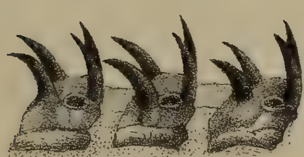

Fig. 4.

Fig. 5.

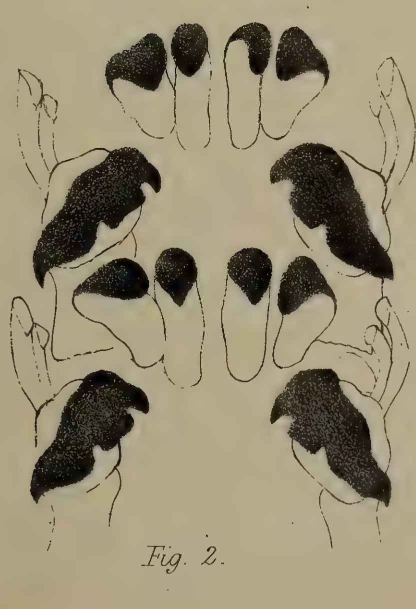

Fig. 2.

T. Alcock del.

W. West lith.

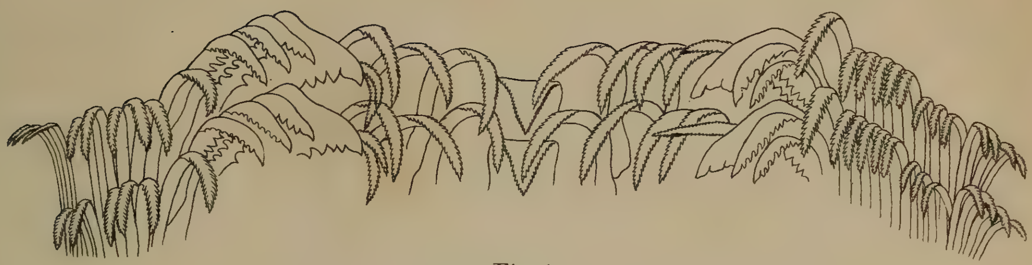

Fig: 1.

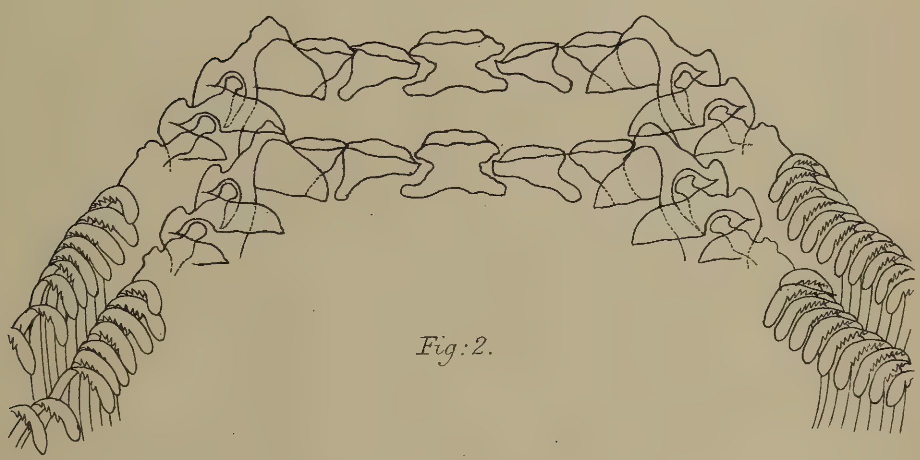

Fig: 2.

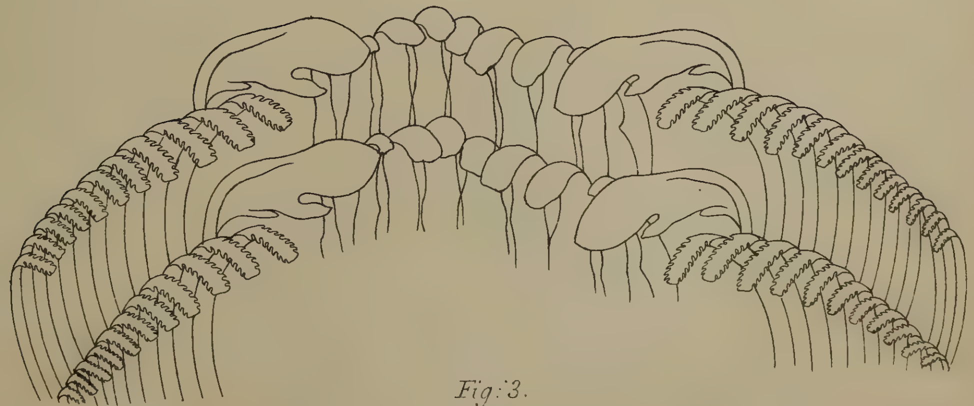

Fig: 3.

T. Alcock, del.

W. West. Lith.

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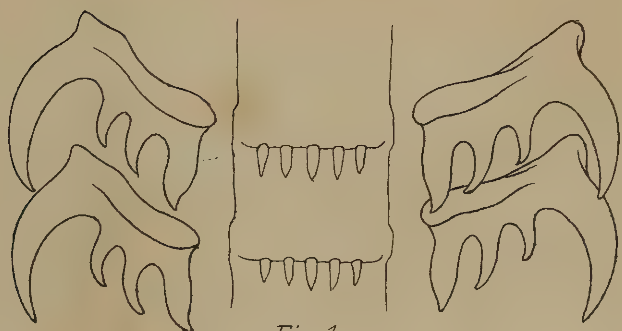


Fig: 1.

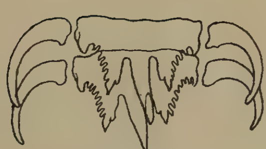


Fig: 2.

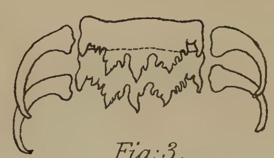
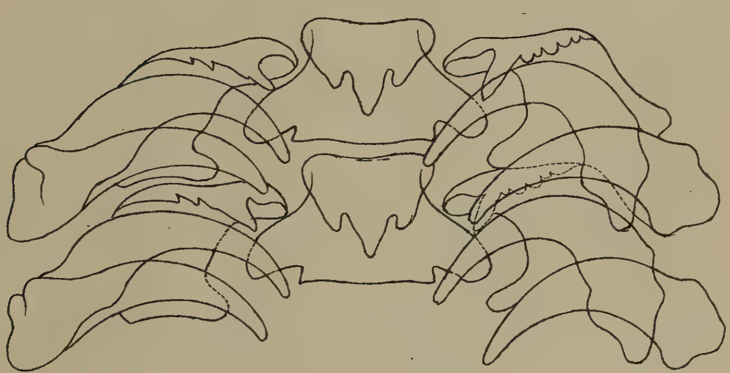


Fig: 3.



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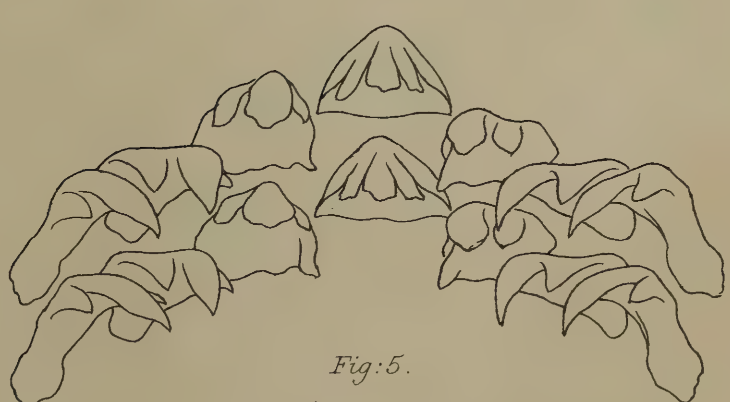

Fig: 5.

T. Alcock, del. W. West Lith.

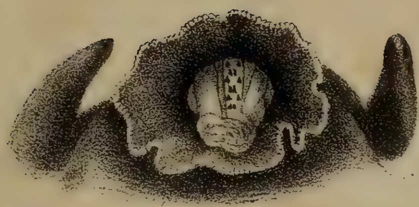

Fig. 1.

Fig. 2.

Fig. 3.

Fig. 4.

Fig. 5.

Fig. 6.

Fig. 7.

T. Alcock del.

W. West lith.

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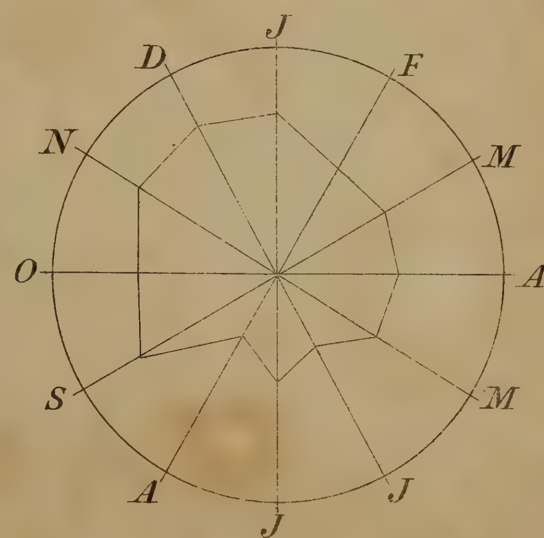
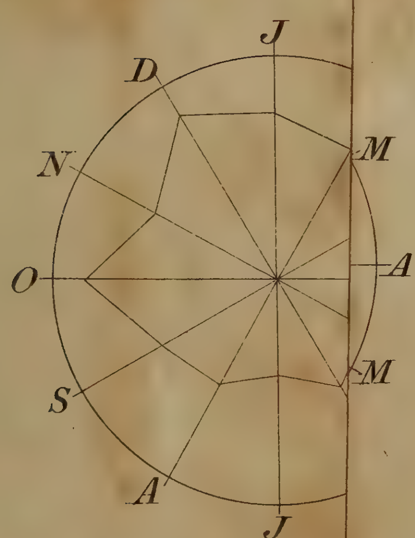
1849–1861.

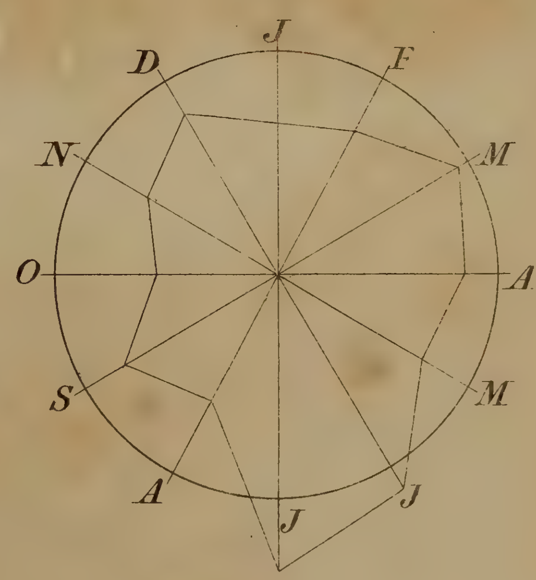
N

S. E





S



N. W

W. West, lith.

ch.

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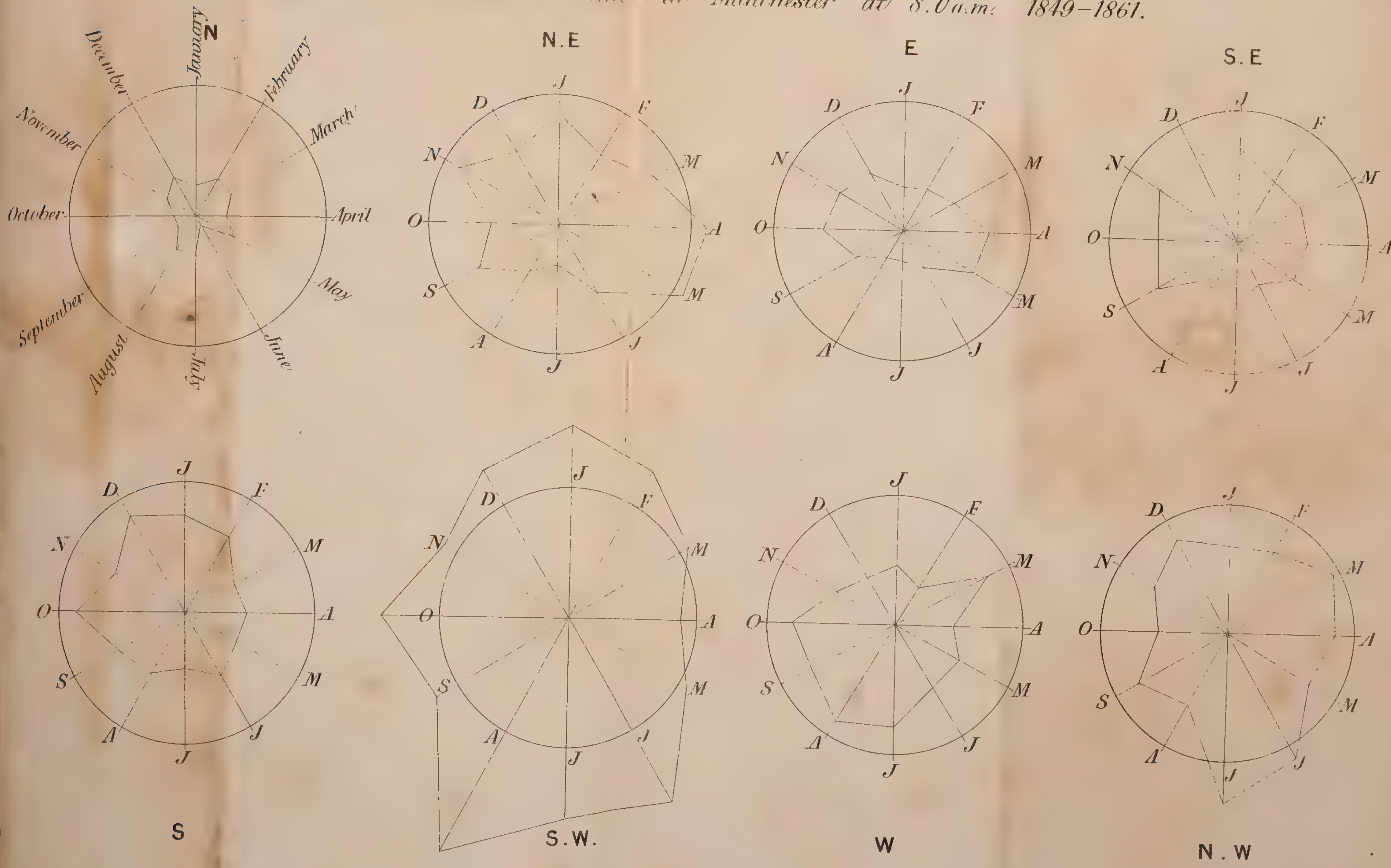
Direction of Wind at Manchester at 8.0 a.m. 1849–1861.

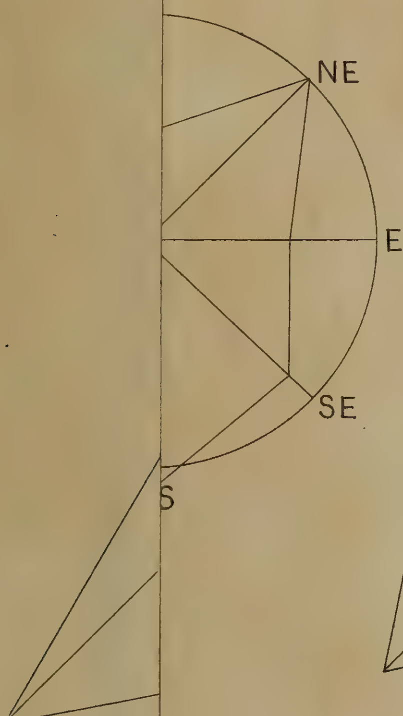


W West, lith.

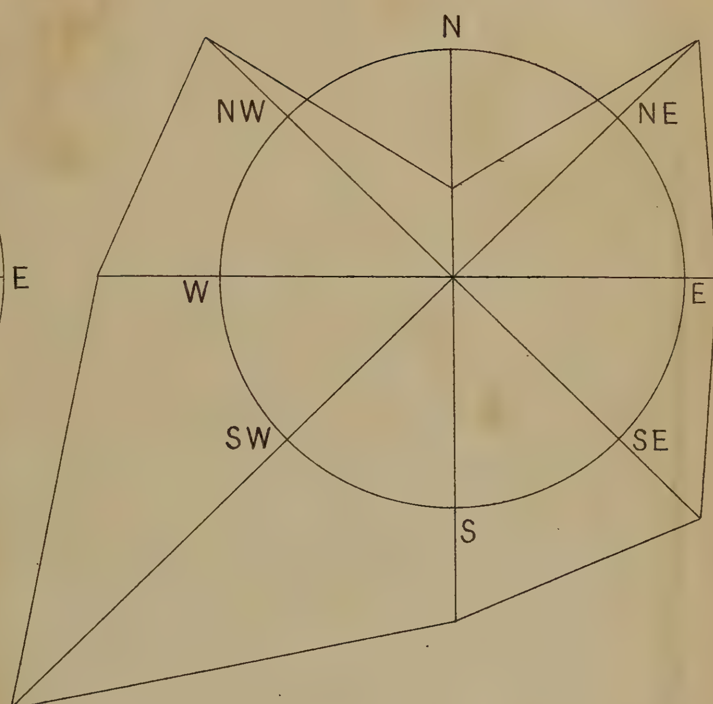
The General Scale of these Diagrams is 6 days to an inch.

ester:

Quarter.



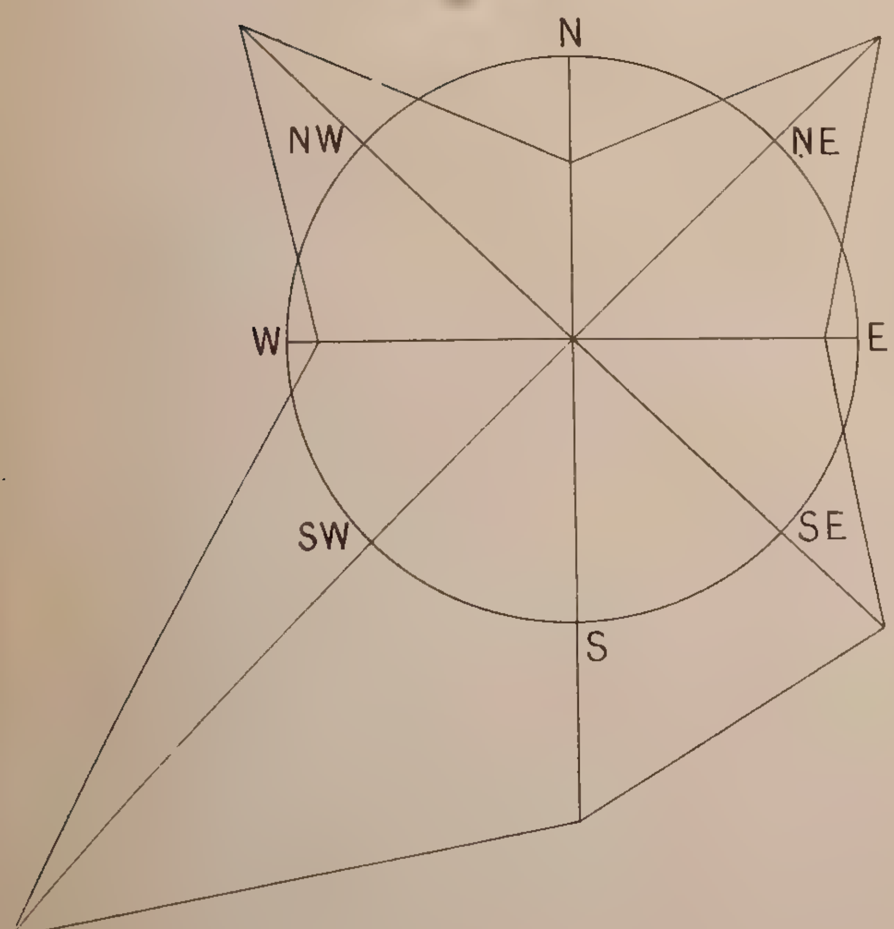
Autumn Quarter.



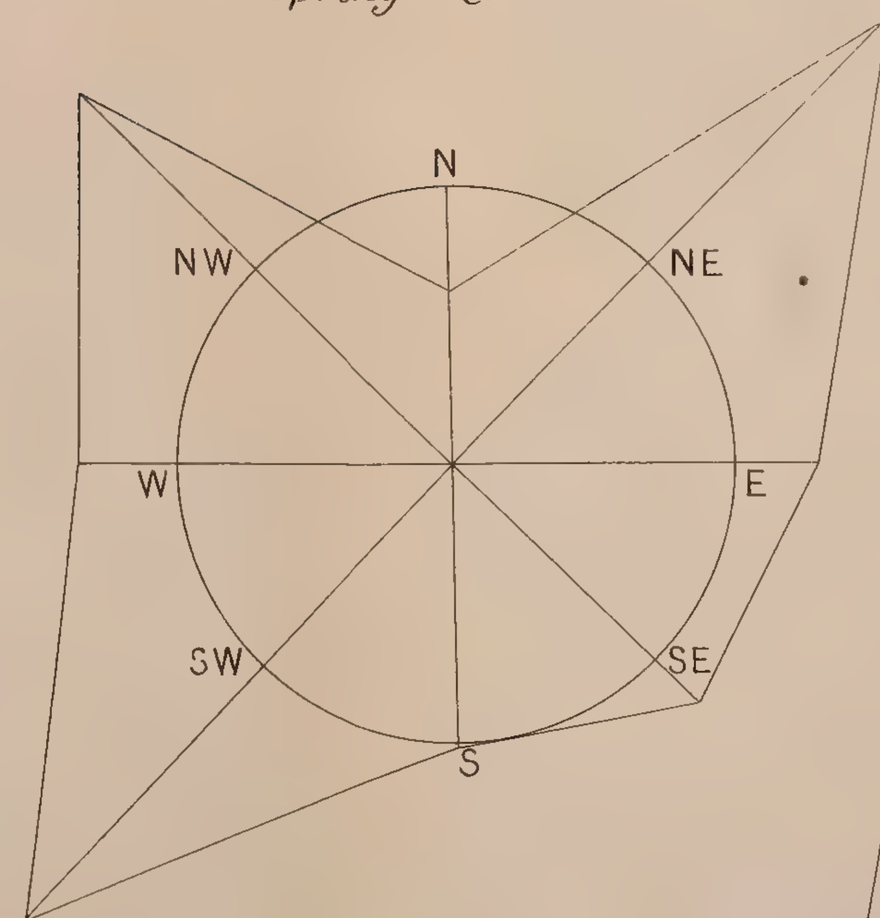
W. West Lith.

Winds in the different Seasons at Manchester.

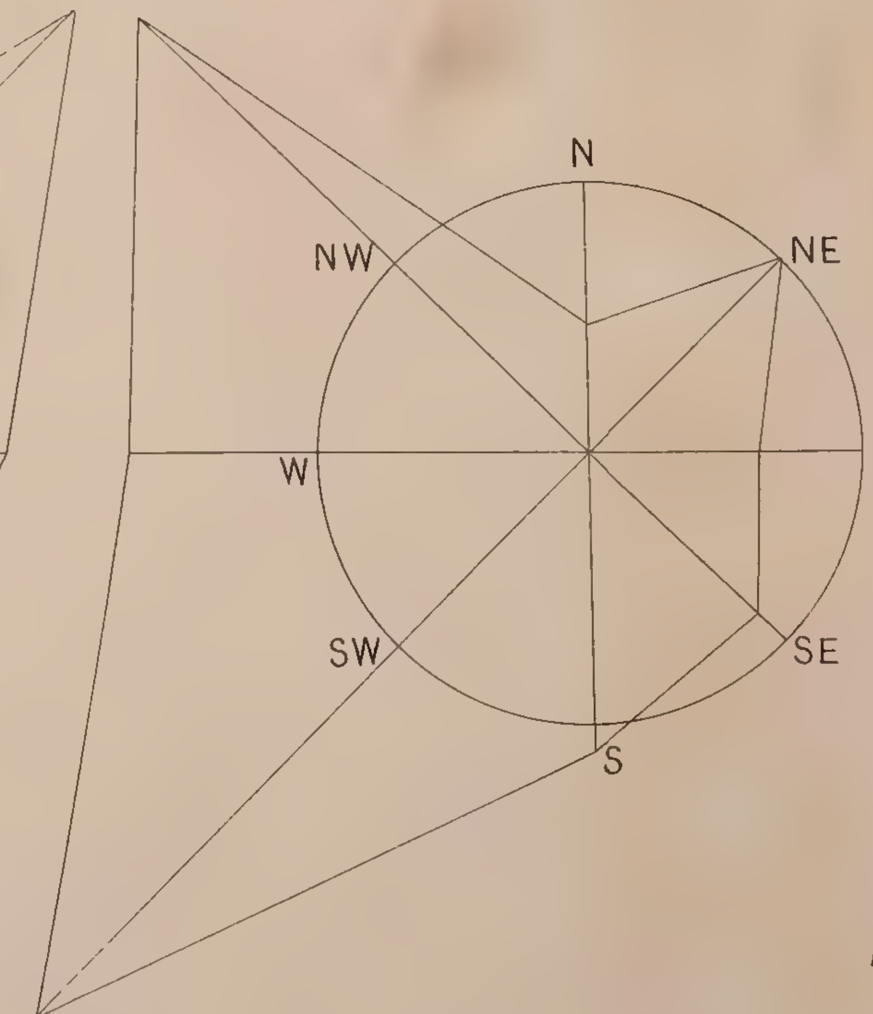
Winter Quarter.



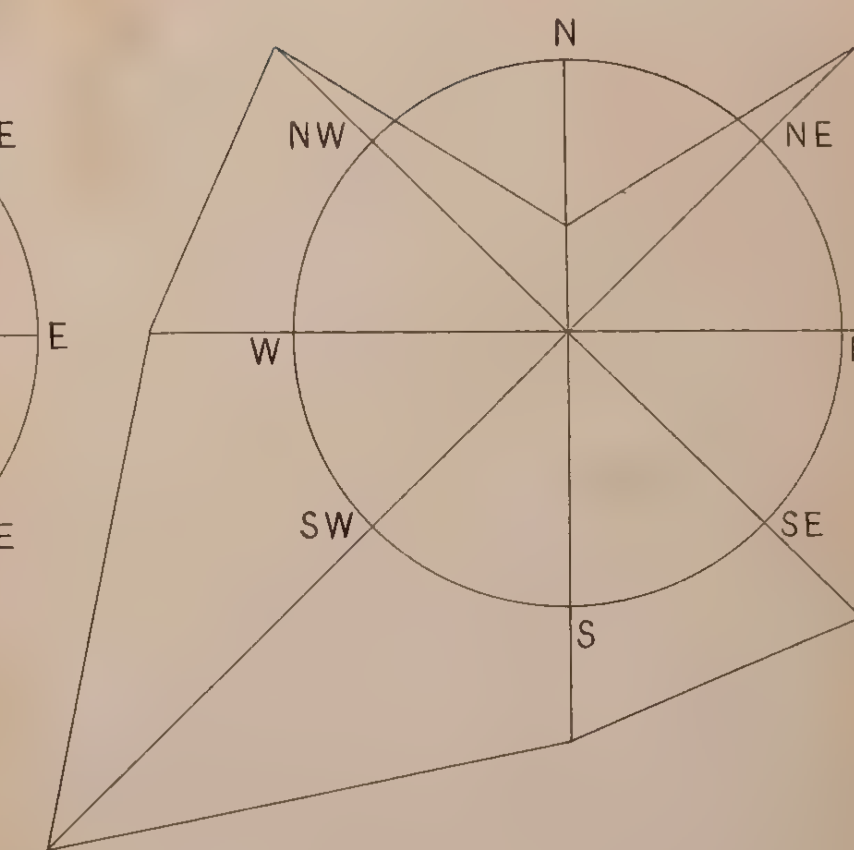
Spring Quarter.



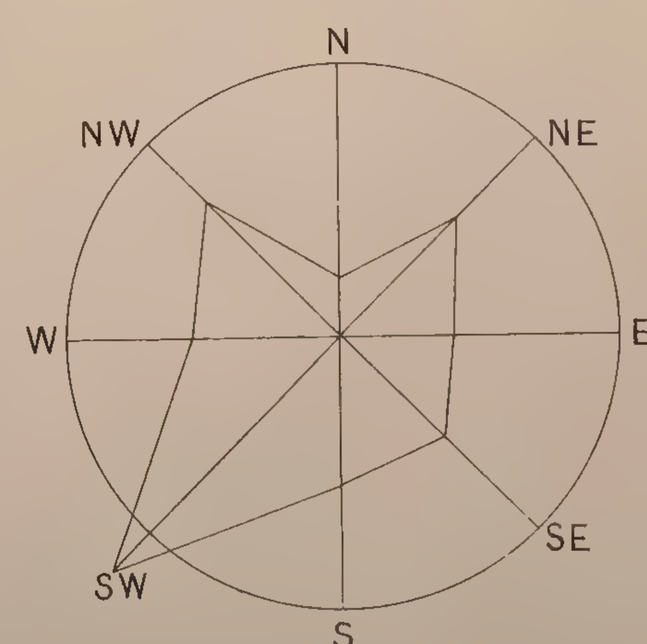
Summer Quarter.



Autumn Quarter.

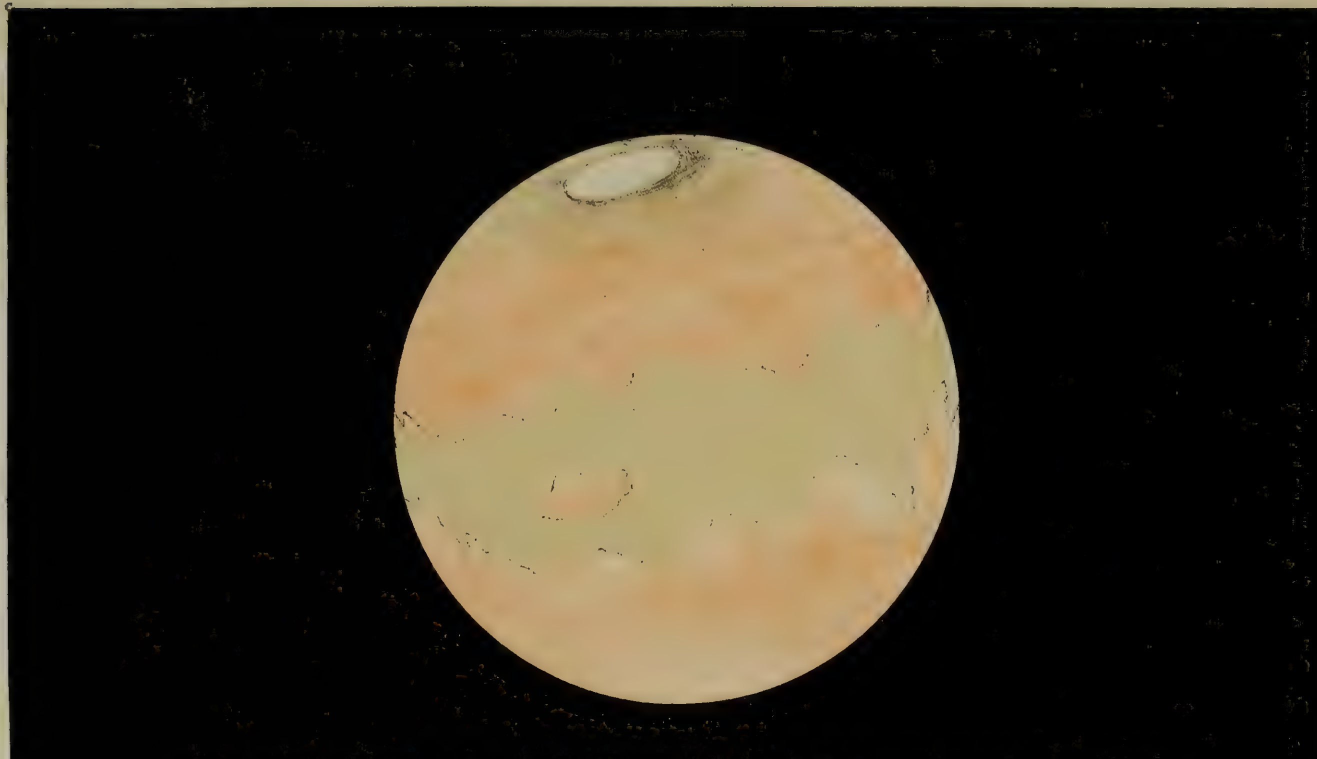


The Year.



One-tenth Scale of other diagrams.

W West Lith.

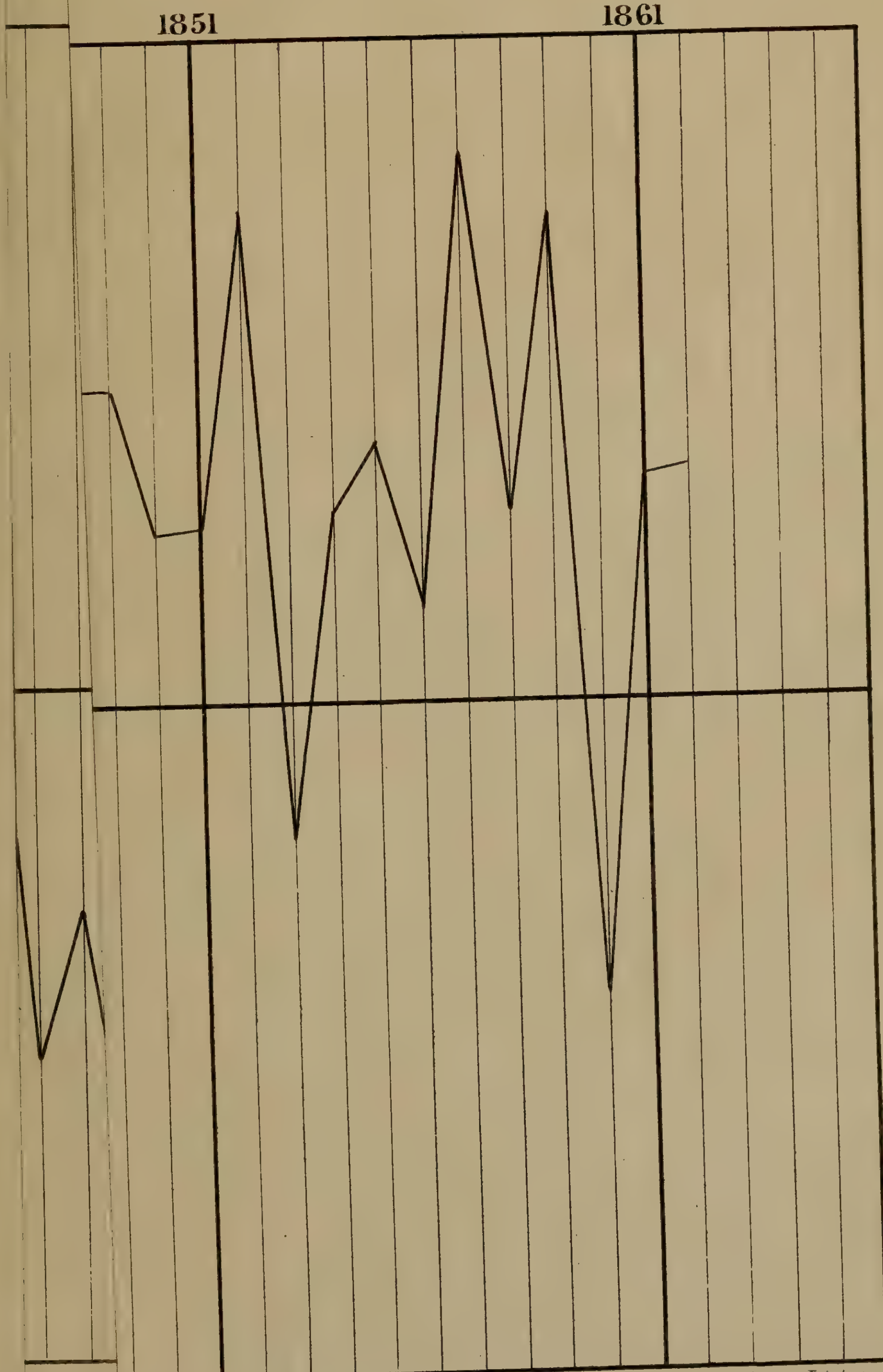


ASPECT OF THE PLANET MARS AS SEEN BY THE AID OF A 20" DIAMR. REFLECTING TELESCOPE

SEPTR. 25 11 OCK. 1862, BY JAMES NASMYTH, AT PENSHURST, KENT.

Mr CARRICK ON THE WAVE OF HIGH WATER.

M^r CARRICK ON THE WAVE OF HIGH WATER.



W. West, Lith.


1771
1781
1791
1801
1811
1821
1831
1841
1851
1861
40°
30°
+ 20°
10°
Mean
10°
— 20°
30°
40°

W. West Lith.